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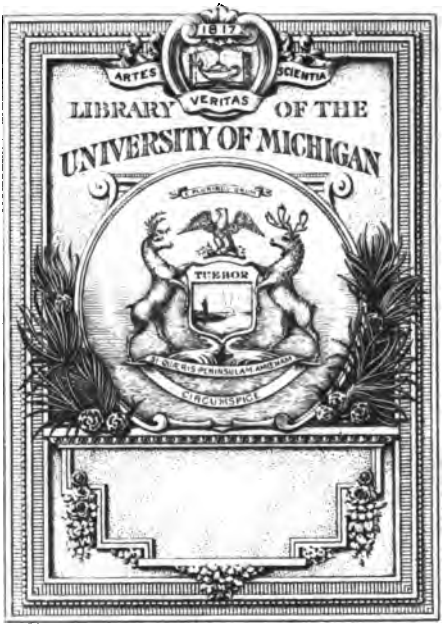
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ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

CHEMISTRY, MINERALOGY, MECHANICS,

NATURAL HISTORY,

AGRICULTURE, AND THE ARTS.

BY THOMAS THOMSON, M.D. F.R.S. L. & E. F.L.S. &c.
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VOL. III.

JANUARY TO JUNE, 1814.

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PREFACE.

THE Editor cannot terminate the Third Volume of the *Annals of Philosophy* without expressing his acknowledgments for the very general and almost unprecedented encouragement and patronage which the work has experienced. When he began to publish the *Annals* he was well acquainted with the state of the scientific publications of this metropolis, and of the difficulty of venturing a competition with works that had been before the public for 16 or 17 years. Indeed he was warned by several of his friends, for whose judgment he has the highest respect, that the ground was pre-occupied, and that success was not to be looked for. He hoped however that, by bestowing an unusual degree of labour and attention upon the *Annals of Philosophy*, and by confining it to original papers, or translations of foreign papers supposed to be unknown to the generality of the British public, he should by degrees establish a journal of so novel a kind as scarcely to be considered as a rival to the other scientific and periodical publications of Great Britain, and claiming attention from the value of the information which it would communicate. The result has shown that this opinion was not without foundation; though the Editor acknowledges with pleasure, and not without a feeling of pride, that the success of the work has been much more rapid, and the attention paid to it by men of science much greater, than he had expected. Of the articles in this volume 115 are original papers which made their first appearance in

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the *Annals of Philosophy*, and 12 are translations of foreign papers which the Editor considered as the most deserving of notice in the journals which he has had an opportunity of perusing.

This is doubtless a very small proportion of foreign papers: yet the Editor has been at considerable pains to procure foreign journals, and has omitted no paper which appeared to him of sufficient value to occupy a place in the *Annals of Philosophy*. Two causes will sufficiently explain the smallness of this number:— 1. Our intercourse with other European nations has for a long time been such that it was impossible to obtain any Italian journals; and the French and most of the German journals, hitherto received in this country, are almost a year behind-hand. 2. The state of the Continent has been such during the last two years that much attention to science could not be expected either in Germany, France, or Italy. Hence the foreign journals are at present unusually meagre: for example, the last two volumes of the Memoirs of the French Institute contain only seven papers, among which there is not one that could with propriety have been inserted in the *Annals of Philosophy;* and for some months past no numbers of the Annales de Chimie have been published. In the termination of the war, and the restoration of the ancient family to the throne of France, the Editor anticipates a new and a happy era for the progress of science, and the general diffusion of knowledge. Within a few months he expects to receive foreign journals regularly, and to find them filled with much more valuable materials than could be expected during a period of general confusion and calamity.

May 27, 1814.

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ANNALS

OF

PHILOSOPHY.

JANUARY, 1814.

Article I.

Sketch of the Improvements in Science made during the Year 1813. By Thomas Thomson, M.D. F.R.S.

Nothing is more pleasing than to take a view of the successive steps by which the different sciences advance towards perfection, and nothing more useful than to observe the various rates at which each of them is advancing in our own time. Such knowledge enables us to appreciate the taste of the age in which we live, and shows us the various branches of knowledge which constitute the fashionable objects of study. I conceive therefore that the sketch, which I am about to present to my readers, of the progress of Science during the year 1813, imperfect and mutilated as it must of necessity be, will not be unacceptable.

The countries towards which we naturally turn our eyes, when we think of the progress of science, are Britain, France, Germany, Sweden, and Italy. Of what has been done in Britain, as far as can be collected from the different Journals and philosophical works published during the course of the year, there is no difficulty of procuring information. Several of the French Journals find their way into this country with considerable regularity; though not till long after the period of publication. Hence what I shall state with respect to that country will rather apply to 1812 than to 1813. Germany has been the theatre of the most bloody war that Europe has yet seen, and cannot therefore be expected to furnish much materials for our historical sketch; especially as Saxony, the country in which some of the most important scientific journals are published, has been occupied by the French army, and of consequence none of these Journals could make their way to London. Sweden, as far as I know, does not furnish any regular scientific journal;

and the state of Italy has for some years nearly interrupted all scientific communication between that country and Britain.

I have made the preceding statement to enable the reader to appreciate in some measure the defects of the following sketch. It may be considered rather as a detail of the progress of science in Britain and France, than of its progress throughout Europe.

I. Mathematics.

This science has now advanced so far that we are not entitled to look for important additions to it every year; yet the last year has been fortunate in this respect, having produced two works, each of them of great importance to the progress of the science.

1. The first is Mr. Ivory's paper on the Attraction of an extensive Class of Spheroids, published in the Philosophical Transactions for 1812. This subject, which is of great importance in physical astronomy, has occupied the attention of mathematicians for these seventy years. Maclaurin resolved it in a particular case in 1740. Lagrange and d'Alembert extended his demonstration. Legendre and Biot endeavoured to generalize it; but without success. At last Mr. Ivory has reduced the subject to a wonderful degree of simplicity, by demonstrating that the attraction of a homogeneous ellipsoid upon any external point whatever may be reduced to that of a second ellipsoid upon a point within it.

2. The second work to which we allude is the Analytical Theory of Probabilities, by Laplace, published at Paris during the year 1812, but not received in this country until the summer of 1813. This book, as might be expected from the profound knowledge of the author, contains much new matter of a very valuable kind; but as I have not had an opportunity of perusing it, I can only refer to the account of it given by Delambre, and to be found in the *Annals of Philosophy*, vol. i. p. 311.

Three other mathematical papers have made their appearance in the Philosophical Transactions, all of them of considerable value. The first, on the Attraction of such Solids as are terminated by Planes, by Thomas Knight, Esq. This investigation has been carried much farther by Mr. Knight than by preceding mathematicians. It claims the attention of chemists; for if ever chemical affinity be brought under the reach of mathematical investigation, it will be necessary to investigate the effect of figure in determining the strength of attraction of different atoms for each other. To Mr. Knight we also owe the solution of a very curious and beautiful problem respecting the penetration of a hemisphere by an indefinite number of equal and similar cylinders. The only other mathematical paper in the Transactions is an application of Cote's theorem by Mr. Herschell, sufficiently curious, but not capable of being explained without entering into details inconsistent with the object of this sketch.

II. Astronomy.

This science has made so much progress, and astronomical observations require so much nicety, and such perfect and expensive instruments, that for many years they have been almost confined to national observatories; among which that at Greenwich has hitherto held the first place, both on account of the importance of the observations, and their being the only ones regularly published. The following are the only astronomical facts, which, to my knowledge, have been published in the course of the year.

1. Mr. Pond has made observations on the summer and winter solstice of 1812, in order to determine the obliquity of the ecliptic. He found it, by the summer solstice, 23° 27′ 51·50″: by the winter solstice, 23° 27′ 47·35″. This small difference, he conceives, may probably be owing to a small error in Bradley's table of refractions, which astronomers have been in the habit of using. He is at present employed in endeavouring to determine that point.

2. Mr. Pond has likewise published a table of the north polar distances of 14 of the principal fixed stars. He conceives his table to be much more accurate than any hitherto offered to astronomers. The maximum of error he thinks seldom exceeds half a second, and only in four cases amounts to one second: for example, the pole star in summer is 1° 41′ 22·07″, and in winter 1° 41′ 21·47″, from the north pole of the heavens.

3. It is well known that the measurement of three degrees of latitude by Col. Mudge, in 1793, in the southern extremity of Great Britain, does not correspond with the measurements made in other countries to represent the earth as flattened at the poles. In Col. Mudge's measurement the length of each degree diminishes as we advance northward, instead of increasing, as had been found in other countries. Various conjectures had been thrown out to account for this anomaly; and Mr. Playfair's opinion, that it might depend upon the vicinity of the sea and the nature of the rocks under the surface of the earth, was considered as very probable: but, in the Philosophical Transactions for 1812, a paper was published by Don Joseph Rodriguez, in which he endeavours to show that the apparent anomaly was owing to errors in the astronomical observations, which occasioned corresponding mistakes in the latitude; and that when these errors are corrected, the apparent anomaly disappears. The paper is remarkable for its moderation and apparent candour; and certainly the suggestion which it contains is well entitled to attention. Unless too it can be shown in the clearest manner that the alleged errors have not been committed, one would naturally suppose that the true mode of settling the point would be again to repeat the astronomical observations.

But Dr. Olinthus Gregory, of the Military Academy, Woolwich, has published a letter on the subject, in a style quite new to astronomical discussions. He affirms that the sole object of Don Rodriguez was to elevate the French astronomers, and depress the English; and he insinuates pretty plainly that the Royal Society concurred in the same design. He then shows that an insular situation is peculiarly ill adapted for such measurements, that the

French observations exhibit greater discrepancies than the English, and that their apparatus was inferior. Finally, he infers, from the goodness of the instruments, and the precautions taken, that the error of Col. Mudge could not exceed half a second. Now all this may be very true; but the point can only be settled by repeating the observations; and till this is done it is obvious that well-founded doubts may remain upon the subject.

4. The comet which made so conspicuous a figure in the heavens during a considerable part of 1811 could not possibly escape the attention of philosophers. A very curious and accurate account of all the particulars observed was drawn up by Dr. Herschell, and is published in the Philosophical Transactions for 1812, p. 115.

A second comet, observed at the end of 1811 and beginning of 1812, is also described by Dr. Herschell in the Philosophical Transactions for 1812, p. 229.

A third comet was observed in July and August at Marseilles and Paris. But it does not seem to have been visible in Great Britain. Its orbit was calculated by MM. Bourard and Nicolet, and it was found not to resemble any other before known.

5. Mr. Dick has found by experience that the planet Venus may be distinctly seen when only 3° from the sun, provided the direct rays of the sun be intercepted from entering the telescope; and he thinks she may be seen even at the distance of 1½°.

6. Some valuable observations respecting the tides have been published by an anonymous writer in Nicholson's Journal, vol. xxxv. pp. 145 and 217.

7. Mr. Ez. Walker has determined the latitude of Lynn, in Norfolk, to be 52° 45′ 24·4″ N., and its longitude 1′ 35·2″ in time E. from Greenwich. Phil. Magazine, vol. xli. p. 331.

III. Optics.

The discoveries in this important branch of science have been curious and interesting. They originated with Malus; and since his death have been prosecuted by Biot and Arrago in France, and by Dr. Brewster in Scotland.

If a ray of light fall upon one of the surfaces of a rhomboid of Iceland crystal, and is transmitted through the opposite surface, it is separated into two pencils, one of which proceeds in the direction of the incident ray, while the other forms with it an angle of 6° 15′. The first of these pencils is said to experience the *usual* or *ordinary* refraction, and the other the *unusual* or *extraordinary* refraction. If the luminous object from which the ray proceeds be looked at through the crystal, two images will be distinctly seen, even when the rhomboid is turned round the axis of vision. If another rhomboid of Iceland spar be placed behind the first, in a similar position, the pencil refracted in the ordinary way by the first will be so also by the second; and the same thing holds with the extraordinary refracted pencil, none of the pencils being separated into two, as before. But if the second rhomboid be slowly turned round, while the first remains stationary, each of the pencils begins to be separated into two; and when the eighth part of a revolution is completed, the whole of each of the pencils is divided into two portions. When the fourth part of a revolution is completed, the pencil refracted in the ordinary way by the first crystal will be refracted in the extraordinary way only by the second, and the pencil refracted in the extraordinary way by the first will be refracted in the ordinary way only by the second: so that the four pencils will be again reduced to two. At the end of $\frac{3}{8}$, $\frac{5}{8}$, and $\frac{7}{8}$ of a revolution, the same phenomena will be exhibited as at the end of $\frac{1}{8}$ of a revolution. At the end of $\frac{4}{8}$ and $\frac{6}{8}$ of a revolution, the same phenomena will be seen as at the first position of the crystals and at the end of $\frac{2}{8}$ of a revolution. If we look at a luminous object through the two rhomboids, we shall at the commencement of the revolution see only two images, viz. one of the least and one of the greatest refracted images. At the end of $\frac{1}{8}$ of a revolution four images will be seen; and so on.

It is obvious that the light which forms these images has suffered some new modification, or acquired some new property, which prevented it in particular parts of a revolution from penetrating the second rhomboid. This property has been called *polarization;* and light is said to be *polarized* by passing through a rhomboid of calcareous spar, or any other doubly refracted crystals.

Some years ago Malus announced the discovery of a new property of reflected light. He found that when light is reflected at a particular angle from all transparent bodies, whether solid or fluid, it has acquired by reflection that remarkable property of *polarization* which had hitherto been regarded as the effect only of double refraction. If the light of a taper reflected from the surface of water at an angle of 52° 45′, be viewed through a rhomboid of Iceland crystal, which can be turned round the axis of vision, two images of the taper will be distinctly seen at one position of the crystal. At the end of $\frac{1}{8}$ of a revolution one of the images will vanish; and it will reappear at the end of $\frac{2}{8}$ of a revolution. The other image will vanish at the end of $\frac{3}{8}$ of a revolution; and will reappear at the end of $\frac{4}{8}$: and the same phenomena will be repeated in the other two quadrants of its circular motion. The light reflected from the water, then, has been evidently *polarized,* or has received the same character as if it had been transmitted through a doubly refracting crystal.

The angle of incidence at which this modification is superinduced upon reflected light increases in general with the refractive power of the transparent body: and when the angle of incidence is greater or less than this particular angle, the light suffers only a partial modification, in the same manner as when two rhomboids of Iceland spar are not placed either in a similar or in a transverse position.

Malus found that light reflected from opaque bodies, such as black marble, ebony, &c. is also polarized: and shortly before his death he ascertained that polished metals polarize light as well as other substances: a discovery which was also made by Dr. Brewster, without his being aware that he had been anticipated by the French philosopher.

When a ray of light was divided into two pencils by a rhomboid of Iceland spar, Malus made these pencils fall on a surface of water at an angle of 52° 45′. When the principal section of the rhomboid, (or the plane which bisects the obtuse angles) was parallel to the plane of reflection, the ordinary pencil was partly reflected and partly refracted, like any other light; but the extraordinary ray penetrated the water entire, and not one of its particles escaped refraction. On the contrary, when the principal section of the crystal was perpendicular to the plane of reflection, the extraordinary ray was partly refracted and reflected, while the ordinary ray was refracted entire.

M. Arrago observed the singular alternations of colour exhibited by plates of mica, selenite, and rock crystal, when they are exposed to a *polarized* ray: and M. Biot has discovered the exact laws of these phenomena, has expressed them by mathematical formulas, and has reduced them all to one general fact, from which all the phenomena may be reduced by calculation. For an account of the labours of Biot in this department of optics we refer to the *Annals of Philosophy*, vol. i. p. 225.

Dr. Brewster's researches have been published in his Treatise on New Philosophical Instruments,* the fourth book of which appears to us by far the most ingenious and important part. Several of the tables of experiments there given are well entitled to the attention of philosophers. We must satisfy ourselves at present with laying before our readers Dr. Brewster's own account of the results of his researches.

"1. It has been ascertained that chromate of lead and realgar have a greater refractive power than the diamond, which has always been supposed to exceed every other body in its action upon light.

"2. The chromate of lead possesses a double refraction, about thrice as great as that of Iceland spar.

"3. The three simple inflammable substances have their refractive powers in the very order of their inflammability.

"4. All doubly refracting crystals possess a double dispersive power, the greatest refraction being accompanied with the highest power of dispersion.

"5. The fluates, viz. fluor spar and cryolite, have the lowest refractive powers of all solid substances, and the lowest dispersive powers of all bodies.

"6. The agate, when cut by a plane at right angles to the laminæ of which it is composed, impresses upon a transmitted ray of light the same character with one of the pencils formed by doubly refracting crystals.

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* We expected to have been able before this time to notice this work in the *Annals of Philosophy*; but a pressure of matter has hitherto put it out of our power to present our readers with an analysis of it.

"7. This property of light, whether communicated by the agate, or by double refraction, or by reflection from transparent bodies, may be destroyed by transmitting the light, in one direction, through almost all mineral substances, and even through horn, tortoise shell, and gum arabic; while in another direction the original character of the ray is not altered. The axis of the substance in which the property is destroyed, I have called the *depolarizing* axis; and the axis in which it is not altered, the *neutral* axis.

"8. Mica and topaz, while they possess, in common with other bodies, the neutral and depolarizing axes, have also axes of a different kind. Each depolarizing axis of the mica is accompanied with an *oblique neutral axis*, while the neutral axis, between the two common depolarizing axes, has an *oblique depolarizing axis*.

"9. When the images of a luminous object are depolarized by the mica, they exhibit, by a gentle inclination of the plate, the most singular alternations of the prismatic colours. The same colours were observed in the topaz; and, in a more perfect manner, in a rhomboid of Iceland spar, which exhibited some new phenomena.

"10. Light suffers a peculiar modification when reflected from the oxidated surface of polished steel, which seems to prove that the oxide is a thin transparent film.

"11. Light is partially polarized when reflected from polished metallic surfaces.

"12. The light reflected from the clouds, the blue light of the sky, and the light which forms the rainbow, are all polarized.

"13. It appears, from a great variety of experiments, that bodies exert a different action upon the different coloured rays, oil of cassia having the least, and sulphuric acid the greatest, action upon green light.

"14. The existence of a third, or a *tertiary spectrum*, has been established by numerous experiments; and a method has been pointed out of employing this spectrum as a measure of the action which different bodies exercise upon the differently coloured rays."

Malus before his death discovered that light obliquely refracted through transparent bodies is likewise polarized, and this subject has been prosecuted by Arrago.

Several curious optical instruments have been contrived by Dr. Wollaston and Dr. Young, which deserve to be enumerated among the improvements in Optics.

Dr. Wollaston's *periscopic camera obscura* is described in the Philosophical Transactions for 1812. It enlarges the field of distinct vision, and is remarkable for that simplicity which characterizes all the inventions of this ingenious philosopher.

His single lens micrometer is described in the Philosophical Transactions for 1813. It is destined to measure the diameter of small bodies, which it does with great accuracy and simplicity. An account of both these instruments has been given in the preceding volumes of the *Annals of Philosophy*.

Dr. Young's *eriometer* is founded upon a different optical principle, but is not less ingenious. His own account of it, and many curious measurements made by it, will be found in the *Annals of Philosophy*, vol. ii. p. 115.

Mr. Ware's paper on the *near and distant sight* of persons, together with Sir Charles Blagden's appendix to it, both published in the Philosophical Transactions for 1813, will probably be considered as more closely connected with medicine than with optics. He has shown that near-sightedness depends in a great measure upon the peculiar habits of the person, that it is particularly brought on by literary pursuits, that it is increased by the use of a concave glass, and that it is not apt to diminish as the short sighted person advances in life.

IV. HYDROLOGY.

The most remarkable hydrological improvements of the year are the hydraulic machines of M. Mannoury Dectot, of which an account has been given in the *Annals of Philosophy*, vol. i. p. 183, and vol. ii. p. 412. These curious machines are, the Intermitting Syphon, the Hydreole, the Oscillating Column, and the Danaide. The oscillating column is the contrivance which displays the greatest originality; but the Danaide seems to be the best adapted for a mechanical moving force, and might be used in certain circumstances with obvious advantage.

Mr. Gough's explanation of the mechanism of *Ebbing and Flowing Wells*, which appeared in the 2d volume of the Manchester Memoirs, published in 1813, is entitled to the praise of novelty, and seems perfectly satisfactory. He ascribes the interruption in the regular flow to a quantity of air which occasionally mixes with the water, and partly choaks up the passage.

It may be worth while to notice some other hydraulical inventions which have been made known to the public in the course of the last year.

Mr. Brunton's pump for raising water from wells or mines while sinking is certainly an improvement; but as it would require a long description to make the improvement completely understood, we shall satisfy ourselves with referring to the Transactions of the Society of Arts for 1812, or to Nicholson's Journal, vol. xxxiv. p. 64, where it is copied from the first mentioned book.

For the same reason we refer to Nicholson's Journal, vol. xxxiv. p. 335, for a description of Mr. Woodhouse's perpendicular lift, employed as a substitute for a lock on the Birmingham and Worcester canal at Tardebig.

V. MECHANICS.

Mr. Peter Ewart, of Manchester, has published a most able defence of the opinion adopted by Leibnitz and his disciples, that mechanical force is measured by the mass multiplied into the square of the velocity. The paper is written with great clearness and precision; but it would be doing it injustice to attempt to exhibit an abridgment of it. We refer those who are interested in such discussions to the 2d volume of the New Series of Manchester Memoirs, published during the course of 1813.

For a similar reason we refer to the same book for Mr. Gough's theorems elucidating the mechanical power called *vis viva* on the continent.

One of the most ingenious and useful mechanical inventions lately proposed in this country is Dr. Wollaston's method of drawing very fine wire. He takes a platinum wire, stretches it in the centre of a mould, and fills the mould with silver. The silver is then drawn out into a fine wire. This wire is dipped into aqua-fortis, which dissolves the silver, and leaves the central platinum wire of extreme fineness. Dr. Wollaston's account of his process is published in the Philosophical Transactions for 1813.

VI. ELECTRICITY.

Electricity is one of those branches of science which, after having been for some time nearly stationary, has made an unexpectedly rapid progress of late years; but the year 1813 has added little to our previous electrical knowledge.

M. Poisson has endeavoured to determine by calculation in what manner electricity is distributed on the surface of conductors. He begins by taking for granted the truth of the hypothesis that there are two kinds of electrical fluids, the particles of each of which mutually repel, while the particles of one fluid attract those of the other. Conformably to this hypothesis, he has calculated the distribution of the fluid on two excited spherical bodies in contact. The result of his calculations comes very near to the experiments of Coulomb. He is employed in extending these interesting calculations to new cases.

Mr. Children's splendid galvanic battery, consisting of 20 pair of copper and zinc plates, six feet in length, and two feet eight inches in breadth, deserves to be noticed, because it is the largest of the kind that has hitherto been put in action. He has not yet laid the result of his experiments before the world. A short account of them will be found in the *Annals of Philosophy*, vol. ii. p. 147.

Mr. Walker has observed that when an excited surface is brought near the top of Bennet's electrometer, but not so near as to produce a spark, the gold leaves diverge in the same state of electricity as the excited surface; but as soon as this surface is removed, the gold leaves collapse, and instantly diverge again in a contrary state: and these changes take place every time that the excited surface is moved to and from the cap of the instrument. See Philosophical Magazine, vol. xli. p. 415. Mr. Singer has observed that this fact had been long known to electricians, and he gives an explanation of it; but it is not necessary to dwell upon the subject, as the whole depends upon a well-known electrical law, that when a substance is brought near an excited body the electricity of the nearest side is different from, but that of the remotest side similar to, that of the excited body. All the attractions and repulsions so familiar to electricians depend upon this well-known law.

VII. Magnetism.

Magnetism has made very little progress for a considerable series of years. Hence we have no reason to be surprised that little has been added to our knowledge of the subject during 1813.

The magnet is liable to two kinds of variation, the annual and the diurnal. We possess very few good observations on the diurnal variation, and therefore have not sufficient data for investigating its cause. On that account the observations of Col. Beaufoy, which have been regularly published in the *Annals of Philosophy*, and which have been made with a better apparatus than any preceding observations, and with every possible attention to accuracy, possess peculiar value, and will probably throw a new light on this obscure subject. It would be premature to attempt to draw any deduction from these experiments till they have been continued for a year. A very slight examination of them, comparing them with the diary of the weather, will satisfy us that heat alone is not sufficient, as Mr. Canton supposed, to account for these diurnal variations.

VIII. Chemistry.

This is the science which has made by far the greatest progress during the course of the year. It will consequently occupy a greater space than any of the preceding. It will be attended with some advantage to subdivide it into its various departments. This will enable us to judge what part of it at present engages the principal attention of chemical philosophers.

1. *Heat.*

Considerable additions have been made lately to the precision of our knowledge of some important phenomena connected with heat and combustion. The newly discovered facts have overturned some of our most ingenious and plausible theories, and have shown us that the philosophy of heat and combustion is not so far advanced as had been conceived.

1. Count Rumford, who has devoted himself to the experimental investigation of heat with much perseverance and success, has lately ascertained how much heat is given out by various bodies during combustion. The following table exhibits the quantity of water that would be raised from the freezing to the boiling point by the combustion of a pound troy of the respective substances:—

White wax	7·2108 lbs. troy
Olive oil	6·8900
Oil of colza	7·0906
Alcohol	5·1400
Sulphuric ether	6·1178
Naphtha	5·5900
Tallow	6·3755

He has determined also the quantity of heat evolved by the combustion of the different woods; from which it appears that the lime-tree gives out the most heat, and the oak the least, during combustion.

2. Delaroche and Berard have made a very complete set of experiments to determine the specific heat of the different gases. The following table exhibits the results which they have obtained:

Specific heat of Water	1·0000
Common air	0·2669
Hydrogen gas	3·2936
Carbonic acid gas	0·2210
Oxygen gas	0·2361
Azotic gas	0·2754
Nitrous oxide gas	0·2369
Olefiant gas	0·4207
Carbonic oxide gas	0·2884
Aqueous vapour	0·8470

3. Mr. Sharpe has shown that the density of steam increases with the temperature at which it is evolved. This accounts for the increase of its elasticity without its being necessary to conceive any alteration in its latent heat. It follows from this that the specific gravity of steam is proportional to its elasticity, or to the temperature at which it is evolved.

At 32° its specific gravity is	0·0046
212	0·6896
252	1·3792
307	2·7584

4. Dr. Delaroche has made some important additions to the doctrine of radiant heat as explained by Mr. Leslie. These additions may be comprehended under the following propositions :—

First Proposition.—Invisible radiant heat may in some circumstances pass directly through glass.

Second Proposition.—The quantity of radiant heat which passes directly through glass is so much greater, relative to the whole heat emitted in the same direction, as the temperature of the source of heat is more elevated.

Third Proposition.—The calorific rays, which have already passed through a screen of glass, experience in passing through a second glass screen, of a similar nature, a much smaller diminution of their intensity than they did in passing through the first screen.

Fourth Proposition.—The rays emitted by a hot body differ from each other in their faculty to pass through glass.

Fifth Proposition.—A thick glass, though as much or more permeable to light than a thin glass of a worse quality, allows a much smaller quantity of radiant heat to pass. The difference is so much the less as the temperature of the radiating source is more elevated.

Sixth Proposition.—The quantity of heat, which a hot body yields in a given time by radiation to a cold body situated at a distance, increases, cæteris paribus, in a greater ratio than the excess of temperature of the first body above the second.

5. These observations favour the notion that light and heat are modifications of each other. The experiments of Berard render this opinion still more probable. He confirmed the experiments of Dr. Herschell, that the heating power of the solar ray increases from the violet to the red end. He found its maximum at the extremity of the red ray; though it still continued perceptible at some distance beyond the visible spectrum. He found that the rays of heat were polarized by reflection, as well as the rays of light. The chemical power was greatest at the violet end, or a little beyond it, as had been previously observed by Dr. Wollaston. There is reason, from his experiments, to conclude, that this chemical power extends over the whole spectrum, though it is too feeble at the red end to be perceptible.

6. The cold produced by the evaporation of liquids has been long known to chemists; and the effect of ether, in particular, was explained many years ago by Dr. Cullen. Dr. Marcet has lately added two new facts to this interesting branch of chemistry. He has found that if a glass tube be filled with mercury, surrounded with a cotton cloth wet with ether, and inclosed in the receiver of an air-pump along with sulphuric acid, in the manner of Mr. Leslie, the mercury speedily freezes if the receiver be exhausted of air. The freezing of mercury may be still more simply performed by the sulphuret of carbon. It is only necessary to surround the mercurial tube with lint moistened with alcohol of sulphur, and then to exhaust the receiver. The mercury immediately freezes.

7. We owe another beautiful fact respecting freezing to Dr. Wollaston. If two glass balls be joined together by a long glass tube, one of them half filled with water, and the whole apparatus be hermetically sealed when exhausted of air, the water in the ball will speedily freeze if the other ball be introduced into a freezing mixture.

8. The freezing of alcohol, said to be performed by Mr. Hutton, of Edinburgh, is conceived to be brought about by compressing air over the alcohol, cooling it as much as possible by means of a freezing mixture, and then suddenly allowing the air to escape.

2. *Definite Proportions.*

For some years back the attention of chemists has been much turned to the important fact that all bodies unite together in certain definite proportions. The truth of this fact appears to me to be put beyond all doubt, by the numerous and precise experiments of Berzelius, Dalton, Davy, and several other chemists, both in this country and on the continent. I cannot here attempt an outline of the doctrine; but must satisfy myself with referring to an essay on the subject published in the 7th Number of the *Annals of Philosophy*, to the essay of Berzelius on the same subject, which is inserted in part in the present Number, and to the two parts of Mr. Dalton's Chemistry which have been for some years before the public. The opinion is of such vast importance, that frequent opportunities will occur during the course of the year of laying observations on it before our readers.

3. *Simple Bodies and their Compounds.*

This head comprehends under it a considerable part of chemical bodies. Hence the number of facts belonging to it is considerable. The following are the most important of them:—

1. Phosgene gas was discovered by Mr. John Davy before the period of which we are writing the history; but it deserves to be noticed on account of its remarkable properties. It is composed of equal volumes of chlorine and carbonic oxide gases condensed into half their bulk. It is colourless; has a strong and disagreeable smell. Its specific gravity is 3·669; and 100 cubic inches, under a mean temperature and pressure, weigh 111·91 grains. Hence it is by far the heaviest gas at present known. It reddens vegetable blues. It combines with ammonia, condensing four times its bulk of that gas, and forming a peculiar neutral salt. Phosgene gas is decomposed by water, and by most metallic bodies. It is an acid body of a very peculiar nature, deserving a much more complete examination.

2. Mr. John Davy's paper on the combinations of chlorine and metals deserves great praise, for the precision with which the experiments were made, and for the many new facts which it exhibits: but to enter upon a discussion respecting the truth of the theory of the author would lead into a field a great deal too wide for an historical sketch like the present. It must be obvious to every chemist that Sir H. Davy's explanation of the muriates, *anes* and *anas*, and the extreme facility with which they are changed into each other without any sensible change in their properties, constitutes the vulnerable part of his theory of chlorine. Indeed, his opinions respecting these bodies cannot be embraced without overturning all the received doctrines respecting the neutral salts, doctrines upon which every thing resembling theory in chemistry is founded. The two opposite hypotheses of chlorine and oxymuriatic acid are liable each to objections, which in the present state of our knowledge it is almost impossible to obviate. As chlorine cannot be decomposed

by any means in our power, Davy's opinion at first sight is more simple, and seems a more correct statement of the phenomena: but, on the other hand, muriatic and oxymuriatic acid, when combined with bases, form most commonly the very same saline bodies; or if there be any difference, the salt formed obviously contains oxygen. Davy gets rid of this difficulty; but it is by suppositions so very violent, and so little supported by analogy, that few chemists, I conceive, could be brought to embrace them in the present state of our knowledge.

3. Sir H. Davy's theory of chlorine has been opposed with much acuteness in two papers published in the *Annals of Philosophy*, by Mr. Henderson and Dr. Berzelius. I do not consider myself as at present entitled to give an opinion on so intricate a subject. For this reason I shall not enter into any discussion respecting the experiment made in the College Laboratory of Edinburgh, to determine whether sal-ammoniac, formed from muriatic acid and ammoniacal gases artificially dried, contains any water; nor make any comment on the opposite statements of Mr. John Davy and Mr. Murray respecting that experiment, as published in Nicholson's Journal. I may perhaps be tempted to resume the subject hereafter.

4. Mr. John Davy's experiments on fluoric acid, published in the Philosophical Transactions for 1812, have been followed up by a set of experiments and conjectures on the same substance by Sir Humphry Davy. Of these I shall give an account in a future Number of the *Annals of Philosophy*. He supposes that the base of fluoric acid is hydrogen, and that the hydrogen is combined with an unknown supporter of combustion, to which he gives the name of *fluorine*. Fluorine unites to bases, like oxygen and chlorine, and forms acids. Thus with silicium it forms silicated fluoric acid; with boron, fluoboric acid; and so on.

5. The experiments of Drs. Berzelius and Marcet on the sulphuret of carbon make us acquainted with the composition of a body possessing new and very curious and unexpected properties. It was originally discovered by Lampadius, who gave it the name of alcohol of sulphur. Clement and Desormes afterwards analysed it, and found it a compound of sulphur and charcoal: but this result was called in question by Berthollet, who maintained that it contained hydrogen: an opinion afterwards confirmed by Berthollet, jun. Still more lately it was examined by M. Cluzell, who conceived it to be a compound of sulphur, carbon, hydrogen, and azote. This induced Thenard and Vauquelin to resume their experiments on it; and both of them ascertained that the only constituents were sulphur and carbon, and that these bodies were united nearly in the proportions of 85 parts of sulphur and 15 of carbon. This agrees very nearly with the results of Berzelius and Marcet, obtained about the same time, and without any knowledge of the experiments of the French chemists.

This substance is obtained by subliming sulphur through red-hot charcoal, condensing the product in water, and rectifying it by distillation at a low heat in a retort. It possesses the following properties. It is a transparent colourless liquid, of the specific gravity 1·272. It has an acrid, pungent, and somewhat aromatic taste. It has a peculiar and disagreeable smell. Its refractive power is 1·645. It boils at a heat of between 105° and 110°, and continues liquid at the temperature of − 60°. It is very inflammable, burning with a bluish flame, and emitting copious fumes of sulphurous acid. It is insoluble in water; but dissolves readily in alcohol, ether, and oils, both fixed and volatile. It dissolves camphor. Potassium burns in its vapour, and is converted into a sulphuret, in which charcoal is deposited. According to the very ingenious and satisfactory analysis of Berzelius and Marcet, sulphuret of carbon is composed of

Sulphur	84·83
Carbon	15·17
	100·00

or of two atoms of sulphur and one atom of charcoal.

It appears from the experiments of Berzelius that sulphuret of carbon combines with alkalies, earths, and metallic oxides, and forms a species of compounds to which he has given the name of *carbo-sulphurets.*

6. During the experiments of Drs. Berzelius and Marcet, they observed that when nitro-muriatic acid is made to act for a considerable time on sulphuret of carbon, at the common temperature of the air, there is formed a substance which has very much the appearance of camphor. This substance is white, has a smell similar to that of oxymuriate of sulphur, and an acrid and acid taste. It melts at a gentle heat, and readily sublimes. It is insoluble in water, but dissolves readily in alcohol and ether, from which it is precipitated by water. It dissolves likewise in fixed and volatile oils. Berzelius found this substance a compound of three acids, in the following proportions:—

Muriatic acid	48·74
Sulphurous acid	29·63
Carbonic acid (and loss)	21·63
	100·00

These proportions approach nearest to 3 atoms of muriatic acid, 1 atom of sulphurous acid, and 1 atom of carbonic acid. Berzelius proposes to call this new compound acid *acidum muriaticum sulphuroso-carbonicum.*

7. Some time ago M. Dulong, a French gentleman, by passing a mixture of oxymuriatic and azotic gases through a solution of sulphate or muriate of ammonia, obtained an oily-looking substance, which has the property of detonating with violence when placed in contact with phosphorus or oils. The formation of this body had

been observed by Mr. James Burton, jun. on exposing oxymuriatic gas to a solution of nitrate of ammonia; but he had made no experiments on its nature. The knowledge of this fact enabled Sir H. Davy to form the substance in question; and a curious set of experiments on its nature was published during last winter in Nicholson's Journal, by Messrs. Porrett, jun., Wilson, and Rupert Kirk. Sir H. Davy prosecuted his original experiments on it, and succeeded in ascertaining its composition. Its properties are as follows:—

I have usually formed it by inverting a twelve-ounce phial, filled with oxymuriatic acid gas in a Wedgewood evaporating dish, filled with a weak solution of nitrate or muriate of ammonia, heated to the temperature of 110°. The liquid rises slowly in the phial. When it has ascended about half way an oily film may be observed on its surface, which, when agitated, falls to the bottom. This is the detonating compound.

It has the colour of olive oil; but is rather deeper. It is fluid; and does not congeal when exposed to the cold produced by a mixture of snow and muriate of lime. Its smell is strong and peculiar, and it excites tears. It is more volatile than ether. Its specific gravity is 1·653. It may be exposed to the temperature of 200° under water without decomposition, but at 212° it explodes with violence. It explodes when brought in contact with phosphorus and oils. In muriatic acid it effervesces and disappears, producing oxymuriatic gas. In nitric acid the gas evolved is azote. In sulphuric acid, oxymuriatic and azotic gas are evolved. It detonates in ammonia. Mercury and copper decompose it; but no other metal hitherto tried. Neither sulphur nor the sulphurets detonate with it; but all the phosphurets detonate with it violently. According to Sir H. Davy, it is composed of four volumes of oxymuriatic gas and one volume of azotic gas, or of

Oxymuriatic gas	91·8
Azote	8·2
	100·0

But Davy's analysis is in some measure hypothetical, and depends entirely for its accuracy on the truth of his hypothesis respecting the composition of muriatic acid and chlorine.

8. Thenard has made some singular experiments on ammoniacal gas, which deserve to be attended to, though it is a very difficult task to explain them in the present state of our knowledge. The gas may be exposed to heat in a porcelain tube without undergoing decomposition; but it is speedily decomposed if iron, copper, silver, gold, or platinum, be put into the tube. None of the other metals tried produced that effect. I conceive these metals to act by increasing the temperature to which the gas is subjected. The other metals likely to be tried would be too fusible to answer that purpose.

9. Sir Humphry Davy has shown that steel does not acquire the well-known colours by the application of heat, unless air or oxygen be present. Hence it is obvious that the colours are owing to oxidation. This fact has been long known at Sheffield, and employed to embellish steel instruments.

10. Berzelius has published a curious and acute dissertation in order to prove that azote is a compound of oxygen and an unknown base, to which he has given the name of *nitricum*. He conceives nitric acid to be a compound of 6 atoms of oxygen and 1 of nitric. He has shown that hydrogen can contain no oxygen; and that ammonia is a compound of hydrogen and azote. The oxygen which he conceives to be present in that alkali he supposes to exist in the azote. We refer our readers to the 10th and 11th Numbers of the *Annals of Philosophy*, where they will find this valuable dissertation. It contains, as usual, various accurate chemical analyses.

These are the most important new facts respecting the simple substances, and their immediate compounds, which have been made known during the course of the last year. It would be easy to point out several other facts; but they are of minor importance, and we have carried this part of our historical sketch to the utmost verge of our limits.

4. *Salts.*

The *salts* are a very numerous and important class of bodies. Several valuable additions have been made either to the number or analyses of these bodies. The following are the principal facts of that nature which have come to my knowledge:—

1. Mr. Dalton has published an analysis of the oxymuriate of lime, a salt originally prepared in the dry way by Mr. Tennant of Glasgow, and used in great quantities by bleachers. Mr. Dalton found that dry oxymuriate of lime is a compound of 2 atoms of lime and 1 of acid. When it is dissolved in water one half of the lime is deposited, and a compound of 1 atom lime and 1 atom acid is dissolved in the water. By age the oxymuriatic acid is changed into common muriatic acid, which injures the value of the salt in a commercial point of view.

2. Berzelius has discovered and analysed several new nitrates of lead, of which an account has been given in a paper by that celebrated chemist published in the 2d volume of the *Annals of Philosophy*, p. 278. The neutral nitrate, or the nitrate before known, which crystallizes in octahedrons, is composed of 100 acid + 205·81 yellow oxide of lead. The three new salts discovered by Berzelius are subnitrates; the first composed of 100 acid + 205·81 × 2 yellow oxide; the second, of 100 acid + 205·81 × 3 yellow oxide; and the third, of 100 acid + 205·81 × 6 yellow oxide. Berzelius calls these salts *subnitrate at a minimum*, *intermediate subnitrate*, and *subnitrate at a maximum*. But these names are not

sufficiently expressive to answer the purpose. They might be called *subbinitrate, subtrinitrate, subhexnitrate.* The prefix *sub* denoting the duplication of the base, while the numerals denote the number of proportions of oxide in the salt.

3. Chevreul has described two nitrates of lead, which he obtained by digesting lead in a solution of nitrate of lead. The first is composed of 100 acid + 450 yellow oxide; the second, of 100 acid + 910 yellow oxide: or the second is a subbinitrite.

4. Mr. Wilson, of Dublin, has described a new compound salt, which crystallizes spontaneously in the residual liquor after the distillation of a mixture of 3 parts common salt, 1 part black oxide of manganese, and 4 parts sulphuric acid, of the specific gravity 1·500, in an iron still with a leaden cover. The salt crystallises in octahedrons, is neutral, and is decomposed by solution in water. According to Mr. Wilson's analysis, its constituents are as follows:

Sulphate of soda	55·47
Muriate of manganese	26·79
Muriate of lead	1·52
Water	16·22
	100·00

There is something problematic about this salt. Hence it would be desirable that it underwent a further examination. The quantity of muriate of lead is so small (not amounting to an atom) that we can scarcely hesitate to consider it as mechanically mixed. The form of the salt indicates a peculiar species. We know at present very few instances of two neutral salts, with different acids and bases, combining together; yet such a combination seems to take place here; for the salt in question must consist of a combination of sulphate of soda with muriate of manganese. This combination seems to exclude a great part of the usual water of crystallization of these salts; for both sulphate of soda and muriate of manganese are remarkable for the great quantity of water of crystallization which they contain.*

5. Chevreul has examined the sulphite of copper, which is a red crystallizable salt, composed, according to his analysis, of

Red oxide of copper	63·84
Sulphurous acid	36·16
	100·00

He obtained also a triple sulphite of potash-and-copper, composed of

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* Mr. Wilson was so obliging as to offer to send me some of the salt; but I was so unlucky as to mislay and lose his address, which prevented me from answering his letter. If this notice should happen to be perused by him, I beg to say that I should be much gratified by receiving a specimen, as I wish to subject it to some further trials.

Red oxide	0·9360
Potash	0·1556
Acid	0·6270
	1·7186

6. From the multiplied researches of chemists, it has been ascertained that the quantity of base necessary to saturate a given weight of any acid must contain a determinate weight of oxygen. Hence the following little table will greatly assist chemists in their experiments:—

100 nitrc acid require	14·66 oxygen
sulphuric	20·02
muriatic	30·49
carbonic	36·68

The following analyses by Berzelius have not yet, I believe, been laid before the British public:—

Nitrate of Barytes	Acid	100
	Base	140
Nitrate of Ammonia.	Acid	67·625
	Base	21·143
	Water	11·232
		100·000
Subnitrate of Copper.	Acid	18·9
	Oxide	66·0
	Water	15·1
		100·0
Subnitrite of Lead	Acid	13·6
	Oxide	80·0
	Water	6·4
		100·0
Nitrite of Lead	Acid	23·925
	Oxide	70·375
	Water	5·700
		100·000
Subbinitrite of Lead	Acid	10·175
	Oxide	89·825
		100·000*

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* See Gilbert's Annalen, 1812, vol. xl. p. 162.

Tartrate of Potash	Tartaric acid	70·45
	Potash	24·80
	Water	4·75
		100·00
Sulphate of Soda	Acid	24·76
	Soda	19·24
	Water	56·00
		100·00
Acetate of Soda	Acid	36·95
	Soda	22·94
	Water	40·11
		100·00
Citrate of Lead	Acid	100
	Base	200
		300
Acetate of Lime	Acid	64·218
	Base	35·782
		100·000
Muriate of Ammonia	Acid	50·86
	Ammonia	31·95
	Water	17·19
		100·00
Sulphate of Ammonia	Acid	53·1
	Base	22·6
	Water	24·3
		100·0
Oxalate of Ammonia	Acid	59·37
	Base	26·88
	Water	13·75
		100·00
Oxalate of Lead	Acid	100
	Base	296·6
Muriate of Barytes	Acid	23·349
	Base	61·852
	Water	14·799
		100·000

Sulphate of Lime	Acid		46
	Base		33
	Water		21
			100
Sulphate of Magnesia.	Acid		100
	Base		50·06
Muriate of Lime	Acid		24·686
	Base		25·711
	Water		49·603
			100·000
Sulphate of Alumina.	Acid		100
	Base		42·722
Sulphate of Iron	Acid		28·9
	Black oxide		25·7
	Water		45·4
			100·0
Sulphate of Zinc	Acid		30·965
	Oxide		32·585
	Water		36·450
			100·000
Sulphate of Copper	Acid		31·57
	Oxide		32·13
	Water		36·30
			100·00
Subsulphate of Bismuth.	Acid	14·5	100
	Oxide	85·5	590
		100·0	
Alum	Acid		34·23
	Alumina		10·86
	Potash		9·81
	Water		45·10
			100·00
Or	Sulphate of alumina		36·75
	Sulphate of potash		18·15
	Water		45·10
			100·00*

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* See Gilbert's Annalen, 1812, vol. xl. p. 235.

5. *Analyses of Minerals.*

The number of chemical analyses of minerals which have been made last year, as far as the subject has come under my knowledge, is uncommonly small; owing, I presume, to the unusual agitation of the Continent.

1. I have given, in the first Number of the *Annals of Philosophy*, Gahn's test of alumina in minerals tried before the blow-pipe. It consists in the lively blue colour which such minerals assume when treated with a preparation of cobalt.

2. Mr. Hatchett has favoured us with a very simple method of separating manganese from iron. It consists in mixing the diluted muriates of these metals with a little ammonia, and then filtering; all the iron remains upon the filter, and the manganese passes through. I have given, in the *Annals of Philosophy*, vol. ii. p. 271, several other methods of accomplishing this separation; but Mr. Hatchett's method has the advantage over them all, in point of facility and cheapness.

3. Dr. Marcet has proposed nitrate of silver as a most delicate test of arsenic when held in solution: the yellow colour of the precipitate being quite peculiar, and appearing when the minutest traces of arsenic exist.

4. According to the analysis of Professor Stromeyer, of Gottingen, the mineral called konite, which occurs at Meissner, is composed of

	Magnesia	32·388
	Lime	15·160
	Oxide of iron	2·962
	Silica	0·530
	Carbonic acid	48·808
	Volatile matter	0·252
Or of	Carbonate of magnesia	68·082
	Carbonate of lime	26·719
	Carbonate of iron	4·417
	Silica	0·530
	Volatile matter	0·252
		100·000

5. Mispickel, or arsenical pyrites, according to Chevreul, is composed of

Arsenic	43·418
Iron	34·938
Sulphur	20·132
Loss	1·512
	100·000

6. An aerolite, or meteoric stone, which fell at Erxleben, in Germany, on the 15th of April, 1812, was analysed by Stromeyer, and found to contain the following constituents:—

Iron	24·415
Nickel	1·579
Sulphur	2·952
Silica	36·320
Magnesia	23·584
Alumina	1·604
Lime	1·922
Oxide of iron	5·574
Oxide of manganese	0·705
Oxide of chromium	0·246
Soda	0·741
Loss	0·358
	100·000

A meteorolite, which fell, in 1807, at Weston, in North America, according to the analysis of Mr. Warden, the American Consul General in Paris, was composed as follows:—

Silica	41
Sulphur	$2\frac{1}{3}$
Chromic acid	$2\frac{1}{3}$
Alumina	1
Lime	3
Magnesia	16
Oxide of iron	30
Oxide of manganese	$1\frac{1}{3}$
Loss	3
	100

7. Mr. Smithson has analysed a saline substance from Mount Vesuvius, and found it composed of the following constituents:—

Sulphate of potash	71·4
Sulphate of soda	18·6
Muriate of soda	4·6
Muriate of ammonia	5·4
Muriate of copper	
Muriate of iron	
	100·0

6. *Chemistry of Vegetable Substances.*

No very important additions have been made to our knowledge of vegetable chemistry during the year, unless the detection of some new vegetable bodies be viewed in that light.

1. Mr. Smithson and myself have ascertained the properties of

ulmin. It turns out to be one of the most common vegetable bodies exuding from various trees, and existing, according to Berzelius, in the bark of most trees. When pure, it is tasteless; sparingly soluble in water and alcohol; not precipitated by acids, gelatine, tannin, or metalline salts; very soluble in the alkaline carbonates, and precipitated from this solution by acids, and by most metalline salts. It would appear to differ somewhat in its properties, according to the tree from which it is obtained.

2. I have examined a red liquid substance from Botany Bay, which turns out to be a combination of a species of tannin and water.

3. Kirchoff, a Russian chemist, while engaged in experiments on starch, to convert it into gum, accidentally discovered that, when long boiled in very diluted sulphuric acid, it is converted into sugar. I have seen a specimen of this sugar made in this country, scarcely to be distinguished in appearance from common loaf sugar.

4. Mr. Brande has shown, by very decisive experiments, that alcohol exists ready formed in fermented liquids, and that it is not formed by the process of distillation, but merely separated from the other ingredients with which it was in combination.

5. Bucholz has shown, by very clear experiments, that camphoric acid differs in its properties from all the vegetable acids at present known.

6. Vauquelin has discovered two new vegetable substances in the bark of the Daphne Alpina. The first is an acrid principle, of an oily and resinous nature, which does not distil over with alcohol, but does with water. The second is a bitter principle, which shoots into white needle-form crystals.

7. In the Notices of the last Number of the *Annals of Philosophy* three new vegetable substances are described; namely *polychroite*, *pircrotoxine*, and *boletic acid.*

It does not seem necessary to recapitulate the properties of these substances here. We refer the reader, for information on the subject, to our last Number.

7. *Chemistry of Animal Substances.*

This department is not so far advanced as vegetable chemistry, The papers published on it have been of considerable importance.

By far the most important paper on animal chemistry is Berzelius's general views of the composition of animal fluids, published in the second Volume of the *Annals of Philosophy.* It may be considered as an abridgment of the *Djurkemi* of the author, published at Stockholm, in two volumes, in the years 1806 and 1808, but entirely unknown in this country till the present abstract appeared during the course of last year. This book may be considered as a system of chemical physiology; and it is certainly the most complete which has hitherto appeared. It contains a great number of new and important facts, and a much more accurate chemical analysis of the different substances in the animal body than is to be found any where else. An English translation of this book would be a great acquisition to British physiologists.

The following are the ultimate results of the analyses given by Berzelius in his views of the chemical properties of the animal fluids.

1. Blood consists of crassamentum and serum. The crassamentum is composed of *fibrin* and *colouring matter*. This colouring matter is of an animal nature, and nearly similar in its properties to fibrin: 100 parts of serum were found to contain the following substances:—

Water	905·00
Albumen	79·99
Lactate of soda and extractive . Muriates of soda and potash ..	8·74
Soda and animal matter soluble only in water	1·52
Loss	4·75
	1000·00

Blood also contains iron; but it does not owe its colour to subphosphate of iron. The phosphates found in the ashes of the blood are formed during the incineration.

2. Lactic acid is not, as the French chemists have endeavoured to prove, a modification of acetic acid, but a peculiar acid, possessed of very different properties from every other. Blood contains no gelatine. Albumen approaches in its properties to fibrin.

3. The secretions contain each a peculiar substance, to which it owes its properties; if this be removed the other substances are the same in all.

4. Bile contains no resin, but a peculiar bitter substance, soluble in water and alcohol, called by Berzelius biliary matter; 1000 parts of bile yield

Water	907·4
Biliary matter	80·0
Mucus of the gall bladder	3·0
Alkalies and salts common to all animal fluids	9·6
	1000·0

5. The constituents of saliva are,

Water	992·9
A peculiar animal matter	2·9
Mucus	1·4
Alkaline muriates	1·7
Lactate of soda and animal matter	0·9
Pure soda	0·2
	1000·0

6. Mucus of the nose is composed of

Water	933·7
Mucous matter	53·3
Muriates of potash and soda	5·6
Lactate of soda and animal matter	3·0
Soda	0·9
Albumen and animal matter insoluble in alcohol, but soluble in water	3·5
	1000·0

7. The humours of the eye contain the following constituents:—

	Aqueous Humour.	Vitreous Humour.
Water	98·10	98·40
Albumen	trace	0·16
Muriates and lactates	1·15	1·42
Soda with animal matter, soluble only in water	0·75	0·02
	100·00	100·00

The lens of the eye contain the following constituents:—

Water	58·0
Peculiar matter	35·9
Muriates, lactates, and animal matter, soluble in alcohol	2·4
Animal matter soluble only in water	1·3
Insoluble membrane	2·4
	100·0

The peculiar matter resembles the colouring matter of the blood, except in the absence of colour.

8. Urine contains the following constituents:—

Water	933·90
Urea	30·10
Sulphate of potash	3·71
Sulphate of soda	3·16
Phosphate of soda	2·94
Muriate of soda	4·45
Phosphate of ammonia	1·65
Muriate of ammonia	1·50
Free lactic acid Lactate of ammonia Animal matter soluble in alcohol Ditto insoluble in alcohol Urea attached to these	17·14
Earthy phosphates and fluate of lime	1·00
Uric acid	1·00
Mucus of the bladder	0·32
Silica	0·03
	1000·00

9. The constituents of skimmed cow's milk are as follows:—

Water	928·75
Cheese, with a trace of butter	28·00
Sugar of milk	35·00
Muriate of potash	1·70
Phosphate of potash	0·25
Lactic acid, acetate of potash, and a trace of lactate of iron	6·00
Earthy phosphates	0·30
	1000·00

Cream, of the specific gravy 1·0244, is composed of

Cream	4·5
Cheese	3·5
Whey	92·0
	100·0

10. Vauquelin has found that egg-shells contain the following constituents: carbonic acid, lime, magnesia, phosphate of lime, iron, sulphur, and an animal matter which acts as a cement.

11. The matter of the brain, according to the analysis of Vauquelin, is composed of

Water	80·00
White fatty matter	4·53
Reddish fatty matter	4·70
Albumen	7·00
Osmazome	1·12
Phosphorus	1·50
Acid, salts, and sulphur	5·15
	100·00

12. From an experiment related in the *Annals of Philosophy*, vol. ii. p. 26, it appears that during an inflammation of the glands of the groin, the quantity of heat evolved was sufficient to heat 8½ lbs. of water, or seven wine pints, from 40° to 212°.

13. A calculus extracted from the urethra of a hog, analysed in the same Number of the *Annals*, p. 59, was found to consist entirely of phosphate of lime.

14. From some experiments on the black liquor ejected by the cuttle fish, published by Mr. Grover Kemp, in Nicholson's Journal, vol. xxxiv. p. 34, it would appear that it consists chiefly of albumen; but nothing is stated respecting the colouring matter of this liquid.

15. From the experiments of Dr. Pearson, it appears that the black matter found in the bronchial glands of adult persons is charcoal.

16. Mr. Brande has published some very satisfactory cases of the

advantages attending the use of magnesia and of acids in certain cases of calculous complaints.

17. Sir Everard Home's theory, that fat is formed in the lower intestines, and that this formation evolves excrementitious matter, is ingenious, and his paper contains some curious facts; but the hypothesis is too imperfectly supported by proofs to be likely to gain credit.

IX. Mineralogy.

This department of science is divided into two branches; namely, *geognosy* and *oryctognosy*: the first of which, owing chiefly to the spirit excited by the Geological Society, has been for some years cultivated in this country with much zeal and success.

1. *Geognosy.*

One of the most interesting additions to this branch of science which has been laid before the British public for some years, is to be found in Von Buch's Travels in Norway, an English translation of which was published during the course of last summer. Intending, as soon as possible, to lay an analysis of this book before my readers, I shall not enter into any details here. The transition rocks round Christiana constitute Von Buch's most important discovery. Here he found *transition granite*, *zircon syenite*, and a beautiful rock, to which he gave the name of *diallage rock*. The greatest part of Norway is primitive, and consists of gneiss.

I found, by traversing a considerable portion of Sweden, that the gneiss extends over the greatest part of Scandinavia. The floetz formations occur at the southern extremity of Sweden, beginning at Helsingburg, and extending eastwards along the sea coast. Several spots occur likewise in West Gothland and Dalecarlia, which I considered, from the rocks composing them, to be floetz rocks: but there can be no doubt that they are similar to several described in Norway by Von Buch, and which he considered as transition. He founded his opinion upon the orthoceratites which exist abundantly in the lime-stone; and this species of petrifaction, in his opinion, characterizes transition lime-stone; but I suspect strongly that this conclusion has been drawn upon too slight grounds. In the first place, nothing can be better characterized than the transition lime-stone of Plymouth; yet I have never heard of any person having observed an orthoceratite in that rock. In the second place, I have been informed by Mr. Greenough that this petrifaction occurs in Ireland in rocks decidedly belonging to the floetz formation; and Lhwydd says that they occur in Oxfordshire, Northamptonshire, and Gloucestershire, where I never heard that any transition rocks had been observed. These facts induce me to doubt whether the occurrence of an orthoceratite in a rock be a sufficient reason for inducing us to consider it as transition. The sand-stone and lime-stone which exist in the rocks alluded to in Sweden differ very much from any transition rocks that I have ever

Nautical Experiments by Col. Beaufoy.

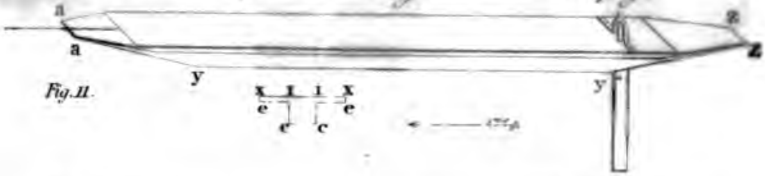

Fig. 11.

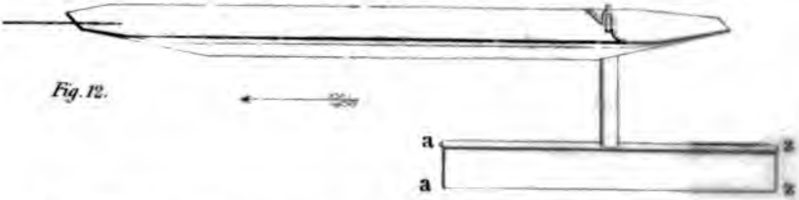

Fig. 12.

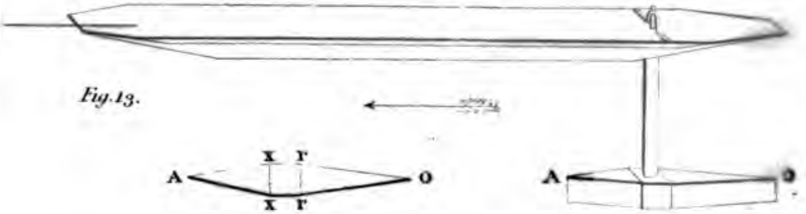

Fig. 13.

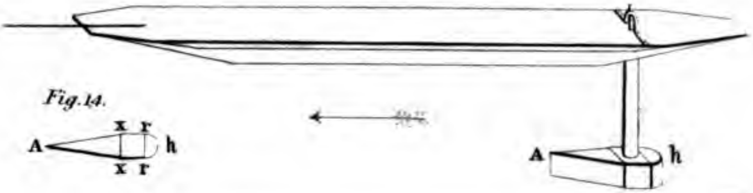

Fig. 14.

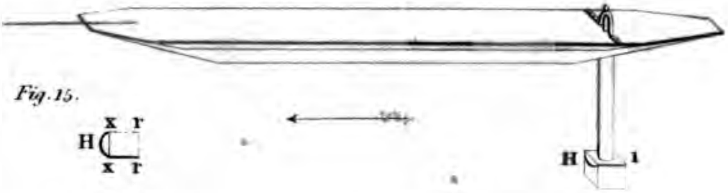

Fig. 15.

Mr. Campbell on the Antilunar Tide.

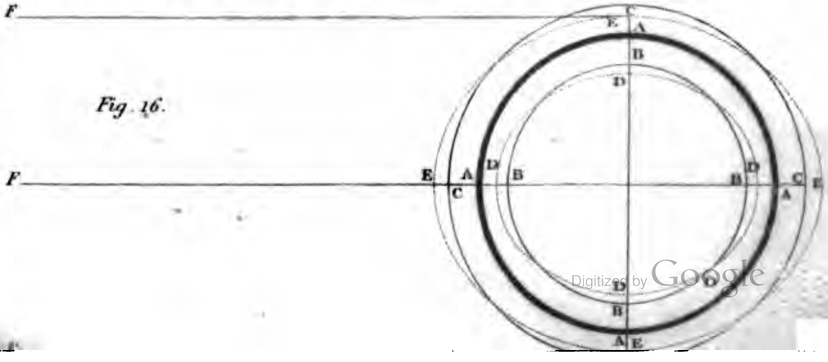

Fig. 16.

seen. The green-stone, however, resembles in its appearance transition green-stone very considerably.

The researches of Cuvier and Brogniart in the environs of Paris have made us acquainted with a new series of floetz rocks, which appear to lie over the chalk, and which succeeding researches have shown to be as common as the formations already known. They have been seen in Spain, in the south of France, in Silesia; and it appears from Mr. Webster's paper lately read to the Geological Society, that these formations constitute almost the whole of the south-east corner of England.

Transition rocks seem to be more abundant in Great Britain than in almost any other country hitherto explored. They constitute the whole south of Scotland, occur in abundance in Cumberland and Wales, and I traced them from Exeter as far west as Penzance, along the sea shore. I found that the serpentine and diallage rock of the Lizzard, and the granite of St. Michael's Mount, belong to the transition class of rocks. The killas of Cornwall seems to be always transition slate, and certainly is never *grey-wacke*.

The Italian valleys of Fiemme, Fassa, and Livinalurga, have been lately examined by Giuseppe Gautieri, and the rocks in them found to consist of floetz trap.

The matrix of the diamond, from a specimen brought to this country by Dr. Heyne, appears evidently to be a species of amygdaloid rock, belonging to the floetz trap formation.

There can be little doubt, from the observations made on different kinds of agates and chalcedony, that vegetable substances occasionally occur in them. Blumenbach lately observed a conferva in a mocha stone; and a plant, resembling sparganium erectum in its fructification, in a remarkable agate brought from Japan.

In the second part of the Philosophical Transactions for 1813 there is a curious paper by Mr. Trimmer, giving an account of the animal remains found in digging two fields near Brentford. The first field, about half a mile north of Kew bridge, consists of the following beds, beginning with the one nearest the surface;—

1. Sandy loam, 6 or 7 feet thick.
2. Sandy gravel, a few inches thick.
3. Loam slightly calcareous, from 1 to 5 feet thick.
4. Gravel containing water, from 2 to 10 feet thick.
5. London clay, about 200 feet thick.

The first bed contains no animal remains; the second contains snail-shells and the remains of river fish; the third contains the horns and bones of the ox, the horns, bones, and teeth of the deer, likewise snail-shells and river fish; the fourth bed contains teeth and bones of the African and Asiatic elephant, teeth of the hippopotamus, bones, horns, and teeth of the ox. The animal remains found in the fifth bed are entirely marine.

The beds and animal remains found in the second field correspond with those in the first.

2. *Oryctognosy.*

The additions to this branch of mineralogy, as far as they have come under my knowledge, have not been very numerous.

1. The description of a collection of minerals from Greenland, by Mr. Allan, published in the second Number of the *Annals of Philosophy*, together with the sketch of the constitution of Greenland written by the same gentleman, and published in the 11th Number, would be read with interest by all mineralogists.

2. The mineral called *lythrodes* by Karsten will be found described in the second Number of the *Annals*. I am not aware that any specimens of it have been brought to this country.

3. Subsulphate of alumina has been found on the south coast of England, first by Mr. Webster, and afterwards by Mr. Smithson Tennant. It is a beautiful white mineral, bearing a certain resemblance to porcelain clay.

4. The turquois has been ascertained to be a peculiar species of mineral, and not fossil bone coloured green, as has been supposed by some.

5. Chromium has been found in chlorite and in serpentine.

6. Titanium has been found by Schrader in graphite.

7. Mr. Holme has analysed arragonite, and found it to contain a portion of water, which does not exist in calcareous spar. In Germany the presence of strontian has been announced in this mineral.

8. The resin found while digging the Highgate Archway appears from my analysis to differ from all other resinous bodies at present known.

9. For the properties and constituents of *pyrodmalite*, I refer to the last Number of the *Annals*.

10. Daubuisson's discovery of the hydrous carbonates of iron, and his new arrangement of the ores of that metal, in consequence of this discovery, is an improvement of some value in this difficult part of oryctognosy.

X. *Meteorology.*

The year 1812 was uncommonly cold. Its mean temperature in London was 49·2°; and the mean temperature of July, the hottest month, was only 61·3°. The mean temperature of December, the coldest month, was 36·5°. The mean height of the barometer in London, at the height of 81 feet above the river, was 29·79 inches. If we suppose that the mean height of the barometer at the sea shore is 30 inches, this would indicate the height of the surface of the Thames at Somerset-house above the sea about 81 feet, which I conceive is greatly beyond the truth. Col. Mudge, in his survey, if I recollect right, reckons it only 7 feet above the Nore. From this it seems to follow as a consequence that the mean height of the barometer at the sea shore is only 29·9 inches.

The rain which fell in London during 1812 is stated in the Philosophical Transactions to be only 22·03 inches: but the rain-gage used stood 102·5 feet above the surrounding ground. Had it been lower, the quantity of rain would have been greatly increased. Another rain-gage, placed 11·5 feet higher, gave only 18·348 inches of rain. I am of opinion that the average quantity of rain which falls in London, if properly taken, would amount to 25 or 26 inches at least.

The fall of rain in Edinburgh during 1812 was 27·112 inches. In Glasgow it was only 22·810 inches; but I have no doubt that the rain-gage in that city, as well as in London, had been placed too high.

The year 1813 was warmer than 1812, though the summer was without any remarkable hot days; but it being necessary to print this paper before the conclusion of the year, it is not in our power at present to draw a comparison between 1812 and 1813.

Two very curious meteorological papers have been inserted in the *Annals of Philosophy*, namely, the mean height of the thermometer at Stockholm for 50 years, and a comparison of the temperature at that place with the corresponding temperature at London, inserted in the 2d Number; and the simultaneous heights of the barometer at London, Paris, and Geneva, for one year, inserted in the 12th Number. We refer our readers to these curious documents themselves for the information which they contain, no less striking than satisfactory.

It does not seem necessary to recapitulate the curious facts contained in Mr. Leslie's late publication on meteorology, after the account which has been given of that book in the last Number of the *Annals*, and of his new meteorological instruments in the 6th Number.

M. Cotte has published some observations upon the Aurora Borealis, in which he has endeavoured to show that it is connected with the increase of the declination of the needle; that it is most frequent when this declination is increasing rapidly, and that it disappears when the declination either ceases to increase or increases in a diminishing ratio. Hence the reason, he conceives, why at present this phenomenon is so seldom observed.

Mr. Thomas Forster published during the course of last year a book entitled Researches into Atmospheric Phenomena, in which he has endeavoured to classify and arrange all the different appearances, and he has given an appropriate name to each.

I have now brought this historical sketch to within two sciences of a conclusion; these are *zoology* and *botany*: but it has imperceptibly swelled to such a length that I cannot with propriety continue it farther. The reader has little occasion to regret the remainder; for as *zoology* and *botany* are conversant chiefly in minute technical descriptions, it would be scarcely possible to do justice to them in a short abstract. Cuvier's account of the labours of the French Institute in these departments of science will be found in the preceding Numbers of the *Annals*; and the zoological papers in the Philosophical Transactions will be noticed when we give an analysis of that work.

Article II.

Remarks on the Hypotheses of Galvanism. By J. Bostock, M.D. M.G.S. &c. &c.

Galvanism may be defined a series of electrical phenomena, produced without the intervention of an apparatus in which the electricity is excited by friction.*

Two hypotheses have been formed to account for the galvanic effects—the electrical and the chemical. The first, which is the hypothesis of Volta, supposes that a peculiar action of conductors upon each other is the first step in the process: the second, that a chemical change in some part of the apparatus is the first step.

An account of the electrical, or, as it is often called, the Voltaic hypothesis, is given by Volta himself in letters which he, at different times, wrote to his friends: 1st, In a letter to Cavallo:† 2d, In a letter to Gren:‡ 3d, In a letter to Sir Joseph Banks:§ 4th, In a letter to Delametherie:|| and 5th, In a letter to Van Marum.** The hypothesis, as stated in the letter to Cavallo, is entirely founded upon the fact, that when metals are placed under certain circumstances, with respect to each other, electricity is produced. The action is denoted by the phrase "destruction of the equilibrium of the metals;" and it is always spoken of as an action by which their natural portion of electricity is altered, one of them becoming positive, and the other negative. The effect produced by the two metals upon each other is the only principle referred to in this paper. In the letter to Gren another principle is brought forward, different from the former. All conductors of electricity are divided into two classes, the dry and the moist; and electricity is supposed to be always excited, when two conductors of one of these kinds are in

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* Is there properly an animal electricity? Can the parts of animals alone, without the co-operation of any other substances, produce electrical phenomena? This would appear to be the case in some of Aldini's experiments; and, if the account be correct, La Grave's animal pile decidedly proves the existence of an animal electricity. (a) What was called by Galvani and Volta animal electricity, is nothing more than electricity excited without the electrical machine, and rendered sensible by its effects upon animals. Volta remarks, respecting these experiments, that the animal is only to be considered as a delicate electrometer. Is the action excited in the experiments of Aldini exactly similar to electricity excited by other means?

† Phil. Trans. 1793.

‡ Ann. de Chim. xxiii. 276 (1797).

§ Phil. Trans. 1800.

|| Nich. Jour. i. 8vo. 135 (1801).

** Ann. de Chim. xl. 225 (1802).

(a) Jour. de Phys. lvi. 235.

contact with one conductor of the other kind. This principle must be considered as essentially different from that which is assumed in the former paper, where the action is conceived to depend upon the effect produced by the contact of two conductors of the same kind, namely, of two metals. In his letter to Sir Joseph Banks, which contains an account of the pile, Volta says, that he abides by his former principles; but he does not say whether he means both his former principles, or which of them. In the letter to Delametherie, there is no reference made to the principle of the two kinds of conductors, as stated in the letter to Gren. The action of the Galvanic apparatus is spoken of as depending upon two metals placed in contact, with a conducting fluid; but this fluid is expressly stated to act only in carrying the electricity from one metal to the other, and not in producing any change in it. In the letter to Van Marum, Volta appears to refer to the first principle only, namely, the action which is excited by two metals; for he brings forward in this paper what he calls a fundamental experiment, which consists in placing a plate of copper and a plate of zinc in contact, but so that a part of the metals may overlap each other, when he finds that of the parts which overlap one becomes positive, and the other negative. It appears, therefore, that Volta has, at different times, brought forward two distinct principles, or hypotheses, which are essentially different, and not necessarily connected with each other; the principle delivered in the letter to Gren, and that maintained in the letters to Cavallo and Van Marum. Mr. Nicholson, having not seen the letter to Gren, conceived of the hypothesis only as it was described in the other papers, and accordingly supposed that it was completely refuted by Sir H. Davy's discovery of an apparatus, which was composed of one metal and two fluids: he expresses his surprise that Volta should adhere to his opinion after this experiment, and concludes that he must have been unacquainted with it;* and the same inference appears to have been made by Sir H. Davy himself.† In the letter to Gren, when speaking of the second hypothesis, that which depends upon the division of conductors into two classes, Volta relates the following fact, as an *experimentum crucis* in favour of his opinion:—A rod of silver and one of tin are each placed in contact with an insulated metallic plate, while the rods are connected together by moisture; the plate in contact with the silver rod becomes positive, and that in contact with the tin rod negative. This is not merely a different arrangement, but it is an experiment essentially different from that which is brought forward as *fundamental* in the letter to Van Marum, as the metallic plates are not in contact, the very circumstance which in the former case was considered the source of the effect produced.

Although Volta appears, from what has been stated, to have brought forward two hypotheses, yet as the first of them is the one which he employs the most frequently, and which seems to be the only one referred to, or recognized by others, I shall speak of this as the electrical hypothesis; and it will be proper to begin by inquiring how far the experiments of Bennett, and others of a similar kind, which have been considered as substantiating, or coinciding with it, can be referred to the same principle.* In the first place, Volta always speaks of the principle as one which he had discovered. In his letter to Cavallo he says, "Thus I have discovered a new law;" "the discovery of this new law, of this artificial electricity hitherto unknown;" and afterwards, "a new and very singular law which I have discovered." Now besides the improbability that Volta would thus disingenuously arrogate to himself the discovery of a series of facts to which he had no claim, there is a stronger reason for believing that the effects of Bennett's experiments are different from the action which Volta supposes to take place in his apparatus, although they have been so generally confounded together. Both from the construction of the pile, and from the direct assertion of Volta himself, it appears that the metals are in contact when this destruction of their electrical equilibrium takes place; but in the experiments of Bennett, Cavallo, and others of a similar kind, the metals that act upon each other were either separated, after having been in contact, or were only in a state of proximity. Bennett never found the different states of + and — to take place while the metals were in contact, but when they were separated, after having been in contact. Fabroni speaks of the effects produced by metals that had been in contact, but were then separated.† Priestley expressly says, that bodies brought near each other acquire different states of electricity; but when they touch, they acquire the same state.‡ It is upon this principle that the Doubler is formed: metallic plates are put in contact, or are placed very near each other, and are afterwards separated, in order that the effect may be produced. The same remarks may be made upon all experiments of a similar nature; and it is obvious that it cannot be in this way that the metals act in the pile, because they are kept in contact during the whole of their action.§ Yet notwithstanding this circumstance, I believe it will be found that all writers have confounded the two principles, or considered them as identical.

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* Nich. Jour. i. 143.

† Phil. Trans. 1801.

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* I think it must have been observed by those who have paid attention to the subject of galvanism, that although Volta has frequently brought forward explanations, or illustrations, of his hypothesis, yet that it has never been done in that ample and detailed manner, which might present an unequivocal view of it in all its parts and relations. This may serve as some apology for any unintentional omission or misrepresentation that may be found in this essay; and will, at the same time, account for the different modifications of it that have been adopted by different writers.

† Jour. Phys. xlix. 350.

‡ History of Electricity, p. 375.

§ An electrified body may communicate electricity either by contact or by approximation. In the first method the electricity communicated is the same with that of the communicating body; by the second, the communicated electricity is of the opposite kind, and is destroyed as soon as the bodies come into contact.

Among others, Sir H. Davy expressly mentions Volta's hypothesis as an extension or generalization of Bennett's experiments, and remarks that Bennett proved that bodies brought into contact, and afterwards separated, exhibited different states of electricity.* Mr. Nicholson always seems to acknowledge their identity: and Mr. Murray, in giving an account of Volta's hypothesis, says, that it proceeds upon the fact that metals acquire different states of electricity, when separated after having been in contact.† It must, however, be considered as still more remarkable, if it should appear that the two circumstances have been confounded by Volta himself; yet this seems to be the case; for in the experiments related in his letter to Delametherie, he examined the metals separately, after they had been in contact, and draws his inference from this examination.‡ These remarks lead us to the conclusion that the experiments which have been brought forward in favour of the electrical action of the metals in the pile are, for the most part, not applicable, that two different principles have been confounded together, and that we have no proof that metals in perfect contact can exhibit different states of electricity: by many experimentalists it is asserted that this cannot be the case.§

Although I think there are strong reasons for this opinion, yet it may not be improper, in the present state of the question, to admit the fact, and to examine respecting the hypothesis, whether it be consistent with itself in all its parts, and how far it explains the different phenomena. Volta clearly lays down the position that the chemical changes produced by the pile are not essential to its operation; that they are only secondary; the effect, not the cause, of the action of the apparatus. He insists that the action between the metals is the essential part of the operation, while that between the metal and the fluid is accidental, or less important; that the fluid serves merely to conduct the electricity of the metals, without producing any change in its state, and that one fluid is preferable to another only in consequence of its being a better conductor. The hypothesis may be thus illustrated (*Fig.* 1). C 1 and Z 1, by their contact, produce a change in their natural quantity of electricity, part of what belonged to C 1 is transferred to Z 1, so that C 1 becomes —, and Z 1 becomes +; and supposing that their natural share of electricity is represented by 100°, and that the copper gives $\frac{1}{10}$ to the zinc, C 1 and Z 1 will be brought to the states of 90° and 110° respectively. The same alteration in their electrical states will, at the same time, take place in the second pair of plates, C 2 and Z 2. The water

Fig. 1.

C 1	Z 1	Water	C 2	Z 2	Water, &c.

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* Phil. Trans. 1807, p. 32.

† Elements of Chemistry, i. 581.

‡ Nich. Jour. i. 136, &c. See also Delametherie's account of Volta's hypothesis, Jour. Phys. liv. 15; and Henry's Elem. i. 243.

§ Cuthbertson, Nich. Jour. ii. 287; Cavallo, as above.

which is in contact with Z 1 and C 2 will, however, from its conducting power, have the effect of equalizing the electrical states of these bodies, and will therefore reduce the electricity of Z 1 to 100°, and raise that of C 2 to the same degree. The electrical state of the four plates will therefore be 90°, 100°, 100°, and 110°. The third, and every succeeding pair of plates, will be acted upon exactly in the same manner with the second; the electricity of the copper, which was reduced to 90° by the action of the zinc in contact with it, will be brought to 100° by sharing a part of the excess which the former zinc plate had acquired. This appears to be all the change which can be produced upon the original hypothesis of Volta; the only fundamental positions of which are, that the electrical equilibrium of two metals is destroyed by placing them in contact, and that the water, when interposed between the plates, conducts the electricity so as to restore the equilibrium. (*Fig.* 2.)

Fig. 2.

	C 1	Z 1		C 2	Z 2	
Original state.	100	100		100	100	
After contact.	90	110		90	110	
After addition of water.	90	100		100	110	

In this state of things the apparatus will be nearly inert, at least it will have no more power than that produced by a single pair of plates; for all the intermediate plates, being in contact with water, will be brought to a state of equilibrium.

What has been stated appears to me to exhibit a fair view of the hypothesis as originally proposed; but several modifications of it have been formed,* for the purpose of accommodating it better to the phenomena. According to one of these, the plates C 1, Z 1, and C 2, Z 2, are supposed, as in the former instance, to have their electrical equilibrium destroyed, while the water tends to equalize the state of the metals on each side of it: but this equalization is counteracted by the action between the metals, which always disposes the zinc to have a certain quantity of electricity more than the copper contiguous to it; and as the middle plates are kept in the same condition with respect to each other by the interposed fluid, C 1 and Z 1 will acquire the states of 85° and 105°, C 2 and Z 2 of 105° and 115°, &c.†

Upon this hypothesis we may make the following remarks:—

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* I have called these modifications of Volta's hypothesis, because they have been advanced as such by the writers who have detailed them; but they are in fact distinct hypotheses, for they all of them proceed upon the assumption of new data.

† This is the way in which Volta's hypothesis appears to be understood by Mr. Nicholson and Mr. Murray, and also by the Committee who made a report to the National Institute. Ann. Chim. xli. 1.

1. That it involves the assumption of a new principle, that the two metals, when in contact, not only effect a change in their respective quantities of electricity, but that this change continues under all circumstances. In this case the zinc plates, which acquire an excess of electricity, are in contact with a conductor; it is therefore reasonable to suppose, that any superabundance of electricity would be immediately carried off by the water, especially when we consider that on the other side of the water we have a set of bodies in the negative state, to which we must suppose that C 2 is reduced in the first instance, and which will consequently be disposed to receive it. 2. This hypothesis supposes that bodies may be differently affected when placed in similar situations. Each pair of plates, except those at the termination, are similarly situated; they consist of two metals in contact with a fluid on the other side of them, so that they ought all of them to be reduced to the same electrical condition, yet the result is that each pair of plates contains 20° more of electricity than the contiguous pair. 3. The quantity of electricity which Z 1 gives to the fluid to be transmitted to C 2 is no more than what is originally lost by C 1 and C 2 having at the same time given exactly the same quantity to Z 2, the result will be that C 2 can only get the quantity which is lost by C 1, so that there can be no progressive accumulation of electricity in the apparatus. 4. Although it be admitted that the tendency to the destruction of the electrical equilibrium still exists in the metals, yet we can scarcely suppose that this tendency should exert itself while they are exposed on each side to the action of a conductor, which must have the effect of restoring the equilibrium as quickly as it is broken. 5. This view of the subject does not account for the actual increase of electricity which seems to be the effect of the apparatus. We have the highest authority for asserting that the action of the pile is augmented by insulation,* so that all the electricity which is brought to the positive end must come from the first copper plate; for with respect to the action which subsists through the body of the pile, between each individual pair of metals, it is merely a reciprocal interchange of a portion of their fluid, without any absolute increase or diminution; it is the first copper plate alone which has lost, and the last zinc plate alone which has gained, any new electricity.

Another method which has been employed for explaining the action of the pile is that which supposes the conducting power of the water to be so much inferior to that of the metals, that although there is a constant tendency in the water to equalize the electrical state of the metals, yet it cannot act with sufficient rapidity; a part only of the excess of electricity in Z 1 can be transmitted to C 2; and instead of both the metals being reduced to 100°, they are left in the state of 105° and 95° respectively. Then by the aid of the former supposition, that each pair of metals, notwithstanding the contact of the water, is in the state of + and − with respect to each other, C 2 being 95°, Z 2 must be 20° more, or 115°.* But this hypothesis is liable to all the objections that were urged against the last, while at the same time it assumes another principle, which has not been proved, that water has not the power of transmitting electricity with sufficient ease or rapidity to prevent its accumulation in any part of the apparatus: and even were we to admit of this retardation, and of the necessity of a certain length of time intervening before the equilibrium could be established, yet this would take place at length, and then the effect of the pile would cease. But we do not find this to be the case: the action goes on without diminution, until either the fluid is exhausted, or the surface of the zinc covered with oxide. We may also remark, both with respect to this and the last modification of the electrical hypothesis, that as all the electricity must ultimately be derived from the first copper plate, it ought to be soon exhausted. There is no source from which the copper plate can receive electricity ab extra, and it is continually transmitting it to the zinc, yet the power of the copper in giving out electricity is never diminished. The electrical hypothesis, in all its modifications, is defective in making no provision for the continued generation of electricity; for, as has already been remarked, the action between each pair of metals is merely a change in the distribution of what they before possessed, not an addition of any new electricity.

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* Van Marum, Ann. Chim. xl. 305, &c.

Sir H. Davy, although originally an advocate for the chemical hypothesis, was afterwards induced to alter his opinion respecting it, and to rank himself among the followers of Volta;† yet it will be found that the ideas which he entertains upon the subject are very far from being the natural deduction from Volta's doctrine, and that he has added to the original hypothesis a new and very important principle. He agrees with Volta in considering the action of the metals upon each other as the first step in the process; but the effect of the fluid in restoring the equilibrium is supposed to depend, not upon its conducting power, but upon the different electrical conditions of the particles of which it is composed, each particle being attracted to that end of the pile which possesses an electricity opposite to itself: he also supposes that the chemical effects are a necessary, although a secondary, step in the operation. So far it will probably be found superior to the other modifications of the electrical hypothesis; but it will, like them, be defective, as affording no adequate explanation of the progressive increase of power in the apparatus; for whether we conceive the equalizing effect of the fluid to depend simply upon its conducting property, or upon the electrical state of its constituent particles, there appears no reason why a perfect equilibrium should not be established between each pair of metals, so as to reduce the action of the whole pile to the sole difference between the first copper and the last zinc plate. It also labours under the objection, which has been already pointed out, that it affords no source for the large supply of electricity which is constantly evolved; like the other hypotheses, it supposes merely a new distribution of the electric fluid through the parts of the apparatus contiguous to each other.*

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* This modification of the hypothesis is that which is adopted by some of the French writers, and also by Volta himself in his letter to Gren. Dr. Henry seems to regard the hypothesis nearly in the same point of view; see Elem. Chim. i. 249.

† Sir H. Davy assigns the following as the reason why he renounced the chemical and adopted the electrical hypothesis. Because he could not refer to any chemical principles the exchange of electricity which the metals experience by contact; because in a pile formed of acid, zinc, and copper, the side of the zinc next the acid is positive, but if a pile be formed of zinc, water, and acid, the surface of the zinc next the acid is negative, the same chemical change thus producing a different electrical effect; and lastly, because many chemical changes are not attended with electrical phenomena. (a) The remarks that have been made will afford a reply to the first of these objections; the second will be answered when the chemical hypothesis is detailed; and as to the third, it may be remarked, that the chemical hypothesis does not render it necessary for us to suppose that electricity is concerned in all chemical changes, nor if it were so, that we should always be able to detect its presence.

(a) Phil. Trans. 1807, § 10.

From these remarks it will appear, that the hypothesis which has been formed to account for the action of the Galvanic pile upon electrical principles is inadequate to the purpose, and must be abandoned. And besides these general views, there are particular facts that seem quite irreconcilable to it, some of which I shall now detail. 1. The experiments which show that the pile has its action suspended in vacuo, or in any gas which does not contain oxygen, is decidedly adverse to the electrical hypothesis; the fact has, indeed, been denied by Volta; † but the weight of authority on the contrary side is too powerful to admit of any doubt. ‡ The experiment of Sir H. Davy may be considered as an extension of this principle, that although in ordinary circumstances a pile cannot act in vacuo, yet that its action may be established by the addition of an acid to the fluid which is interposed between the plates. I am not aware of any method by which these experiments can be reconciled to the electrical hypothesis. 2. It has always been an acknowledged difficulty in this hypothesis to explain the reason why saline fluids are more powerful in their operation than pure water. Volta's method of replying to it, that saline fluids act better only so far as they are better conductors,* has generally been thought unsatisfactory, and yet no other has been substituted in its place. That it should be still persevered in seems the more remarkable, because Sir H. Davy, in an early stage of the investigation, clearly stated that the action of the different fluids was not in proportion to their conducting power.† Indeed, when we consider how readily pure water transmits the electric fluid, we can scarcely attribute to a deficiency in its conducting power the comparatively small effect which it produces in the pile, nor to a mere increase of this conducting power, the vastly greater activity of diluted acids or neutral salts. But independently of all considerations of this nature, it would appear, from referring to the principles of the hypothesis, that the more we increase the conducting power of the fluids, the less should be the action of the pile. The contact of the metals destroys the electrical equilibrium; and, according to Volta, the only effect of the interposed fluid is to counteract this disturbance, and to restore the equilibrium; it must then obviously follow that the more perfect the conductor, the more completely will the equilibrium be restored, and therefore the less effect will be produced by its destruction. The same kind of argument, but with some modification, may also be urged against Sir H. Davy's peculiar hypothesis; for whether we suppose the effect of the fluid in restoring the equilibrium to depend upon its conducting power, or upon its tendency to decomposition, it will follow, that the better it conducts, or the more readily it is decomposed, the less will be the effect of the apparatus; this conclusion is, however, directly at variance with the fact.

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* Although this seems to be the fair deduction from Sir H. Davy's principles, yet he has, on one occasion, given an explanation of the action of the pile, which is considerably different. Supposing the elements to consist of copper, zinc, and fluid, the copper and zinc, by their action upon each other, become respectively negative and positive. Between the zinc and the second copper plate the fluid intervenes; and he says, "with regard to electricities of such very low intensity, water is an insulating body;" (*a*) therefore no action can take place between the first zinc and the second copper plate across the fluid. According to this, which must be regarded as quite a new hypothesis, and one directly the reverse of that adopted by Volta, the metals of each pair of plates are individually plus and minus; but each pair can have no action upon the neighbouring pairs; and the worse conductor the fluid is which is employed, the more complete will be the action of the instrument.

† Ann. de Chim. xlii. 281. ‡ Haldane, Pepys, Biot, Van Marum, &c.

(*a*) Phil. Trans. 1807, § 9.

3. According to the electrical hypothesis, under every one of its modifications, it is necessary to suppose that a current of electricity exists which constantly passes from the first copper plate to the contrary end of the pile; and, indeed, it is expressly stated by Volta himself, that this current constitutes the essential part of its operation.‡ Yet we know that the action of the pile is very considerable before its extremities are united; and what, in this case, are we to conceive respecting the current? Whence does it proceed, and where does it terminate? The supposition that a current must exist at a time when the ends of the pile are not in a situation for either giving or receiving electricity, seems almost decisive against the hypothesis; and it is remarkable, that many of the writers upon the subject, not excepting Volta himself, § in opposition to the clearest evidence of facts, have asserted, or insinuated, that the apparatus does not act until its extremities are united.

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* Nich. Jour. i. 139.

† Nich. Jour. iii. 135. See also his later experiments, Phil. Trans. 1807.

‡ Phil. Trans. 1800, &c.

§ Nich. Jour. i. 135. See also De Luc, in Nich. xxvi. 115, &c.

4. Another objection to the electrical hypothesis is, that it leaves unexplained one of the most important effects attendant upon the action of the pile, the decomposition of the interposed fluid. The fluid is supposed by Volta to act merely as a conductor, and its effect as a conductor is well illustrated in the experiment of De Luc, where he states the result of what he calls the third dissection of the pile. The copper and zinc are here in contact with each other, and the moistened card is placed upon the copper; between this and the next zinc plate is a small wire frame, which prevents the moisture from acting upon the zinc, yet at the same time is sufficient for conducting the electricity which is liberated, because a similar kind of wire is employed for connecting the extremities of the apparatus; yet in this case no electrical effects were produced.*

5. The experiment performed by Sir H. Davy, in which he reversed the action of the pile, by applying different fluids to the same metal, has always appeared to me a very powerful objection to the electrical hypothesis, and seems originally to have been thought by Sir H. Davy himself to be decisive against it. If, according to the direct assertion of Volta, fluids differ from each other solely in consequence of their being better or worse conductors, it would seem evident that different fluids can produce no difference of effect except in degree: the restoration of the equilibrium may proceed with greater or less rapidity, but the nature of the change effected must still remain the same.

6. According to that modification of the electrical hypothesis, which supposes a progressive increase of power in the apparatus, as we advance from one extremity to the other, (a modification which is absolutely necessary to explain the phenomena,) it follows, that each plate is negative or positive, not merely as respects the contiguous plate, but that the whole of one end of the pile is negative, and the other positive, while the plates in the centre are in their natural state.† To refer to the numerical illustration, the metals will be as expressed in *Fig.* 3.

Fig. 3.

C 1	Z 1		C 2	Z 2		C 3	Z 3		C 4	Z 4
		Water			Water			Water		
80	90		90	100		100	110		110	120

It appears, then, that not only C 1, but also Z 1, is negative; and that not only Z 4, but likewise C 4, is positive; while Z 2 and C 3 are both neutral. But the existence of this state of things is not countenanced by the effects which we observe to take place: for according to this arrangement it would be impossible for the apparatus to act if it were disposed in a circle; and it also follows, that if we were to break the chain between Z 2 and C 3, we should find these plates in equilibrio, and consequently no effect could be produced upon any substance interposed between them.

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* Nich. Jour. xxvi. 126.

† De Luc, in Nich. Jour. xxvi. 115, &c.

(To be continued.)

ARTICLE IV.

Some Account of a Set of Experiments made at Greenland Dock, in the Years 1793, 1794, 1795, 1796, 1797, *and* 1798. By Col. Mark Beaufoy; Capt. James Scott, of the First Regiment of Royal Tower Hamlets Militia; and Capt. John Leard, Commanding Earl Stanhope's vessel Ambo.

THESE experiments were at first conducted under the direction, and at the expense, of the Society for Improving Naval Architecture; and after the dissolution of that society, they were continued at the expense, we believe, of Col. Beaufoy. The object in view was to determine experimentally what particular forms of bodies move through the water with the least resistance, both at the surface, and when immersed to a certain depth under the surface. It is well known that the theoretical investigations of this important subject have led to very little of practical value: so that ship building is still, in a great measure, an empyrical art; and no person can pretend to foretell, with certainty, whether a new built ship will sail ill or well. Hence experimental results are the only ones to be depended on in practice. The experiments in question having been made with great care and perseverance, and having been very much varied, as far as relates to the form of the moving body, and to the velocity of its motion, cannot fail to prove acceptable to practical men; and, if properly attended to, might introduce some very important alterations and improvements in the forms of our ships, and the method of building them.

These experiments are so numerous, that it would be impossible to introduce them all into a work of this sort without devoting to them the whole of several successive volumes, a sacrifice which we could not make consistently with propriety. All that we shall attempt, therefore, at present, will be to exhibit a view of the weight necessary to move each of the substances tried with one determinate velocity: though, perhaps, hereafter we may be tempted to exhibit a more complete detail of the experiments respecting such of the forms as seem to move with the greatest or with the least resistance.

The apparatus was very simple and well conceived. The cord fixed to the body to be moved in the water passed under a light wheel, and from that to the top of a three-legged stand, where it was attached to a system of pullies, to which a box was hung to contain the weights necessary to put the body in motion. As it was found that the motion of the body in the water was at first very slow, compared with the velocity which it afterwards acquired and preserved, an additional weight was made at first to act for some time, in order to bring it the sooner to its maximum velocity; but it being found that when this weight suddenly ceased to act, a tremulous motion was produced which interfered with the regularity of the body's progress, a chain was substituted for the additional weight, which reaching the ground in succession, its action was gradually withdrawn without producing any such injurious effect.

The time was measured by means of a pendulum clock, to which there was attached a batten moving over a long horizontal frame accurately divided into parts, and by means of a spring a pencil-mark was left on the frame at the end of every second or interval required. By this peculiar contrivance it was found easy to divide a second into 1000 parts, and to determine the motion to the 1000th part of a second, a degree of precision never before attempted, far less obtained in any similar experiments. The apparatus underwent successive alterations before it reached its greatest perfection; and on that account the earlier experiments are not entitled to the same degree of confidence as those made at a later period. In the following table the experiments are placed in the order in which they were made.

I. *Bodies Floating on the Surface of the Water.*

1. A parallelopipedon of wood. Length, 42·198 feet. Breadth, 3·668 feet. Depth, 1·219 feet. Area of the end, 4·4713 feet:
Velocity, 12 f. per second. Motive weight, 947·23 lbs. Avoir.

2. A similar parallelopipedon of half the length:
Velocity 12 f. per second. Motive weight, 797·42 lbs.
——— 6 ——— ——— 155·96

3. The first parallelopipedon on its edge:
Velocity per second, 12 f. Motive weight, 939·74 lbs.

4. The first parallelopipedon lengthened by adding a semicircular end to each extremity:
Velocity per second, 12 f. Motive weight, 693·1 lbs.

5. A triangular piece of wood with its base foremost. Length of perpendicular, 43·125 feet. Breadth, 4·7416 feet. Depth, 1·219 feet. Area of the base, 5·78 feet:
Velocity per second, 12 f. Motive weight, 1041·70 lbs.

6. The same triangle with its vertex foremost:
Velocity per second, 12 f. Motive weight, 395·50 lbs.

7. A parallelopipedon and triangle joined together, the parallelopipedon foremost. Length of parallelogram, 10·333 feet. Breadth, 3·668 feet. Depth, 1·219 feet. Area of the end, 4·4713 feet. Length of triangle, 32·79 feet:
Velocity per second, 12 f. Motive weight, 801·95 lbs.

8. A parallelopipedon of the same length, but of twice the breadth of that used in experiment 2:

Velocity per second, 6 f. Motive weight, 342·38 lbs.

9. The same parallelopipedon placed on its edge:

Velocity per second, 12 f. Motive weight, 1482·8 lbs.
———————— 6 ———————— 369·27

10. The same parallelopipedon lengthened by a semicircular end at each extremity:

Velocity per second, 12 f. Motive weight, 1460·8 lbs.

11. An isosceles triangle with its base foremost. Length, 30·333 feet. Breadth, 3·375 feet. Depth, 1·21875 feet. Area of the base, 4·113 feet:

Velocity per second, 12 f. Motive weight, 748·84 lbs.

12. The same triangle with its vertex foremost:

Velocity per second, 12 f. Motive weight, 290·70 lbs.

13. The compound body used in experiment 7 having the parallelopipedon end formed into a semi-ellipsis, the elliptical end foremost:

Velocity per second, 12 f. Motive weight, 265·70 lbs.
———————— 6 ———————— 58·277

14. The same body having the parallelopipedon end rounded off into two segments of circles:

Velocity per second, 6 f. Motive weight, 36·642 lbs.

15. A parallelopipedon. Length, 42·198 feet. Breadth, 1·219 feet. Depth, 1·219 feet. Area of the end, 1·4859 feet:

Velocity per second, 12 f. Motive weight, 272·20 lbs.

16. A parallelopipedon. Length, 21·099 feet. Breadth, 1·219 feet. Depth, 1·219 feet. Area of the end, 1·4859 feet:

Velocity per second, 12 f. Motive weight, 257·51 lbs.

17. The same body lengthened by adding a semi-ellipsis to its hindmost extremity. Length of the semi-ellipsis, 3·6058 feet:

Velocity per second, 12 f. Motive weight, 254·14 lbs.

18. The same figure with the semi-ellipsis foremost:

Velocity per second, 12 f. Motive weight, 146·23 lbs.

19. The same figure with a semi-ellipsis added to each extremity:

Velocity per second, 12 f. Motive weight, 154·27 lbs.

20. The parallelopipedon used in experiment 6 lengthened by having a circular wedge added to its hindmost extremity. Half length of the chord, 3·605 feet. Versed sine, 0·6095 foot:

Velocity per second, 12 f. Motive weight, 248·95 lbs.

21. The same as the last with the semicircular wedge foremost:

Velocity per second, 12 f. Motive weight, 141·57 lbs.

22. The same parallelopipedon with a circular wedge added to each extremity:

Velocity per second, 12 f. Motive weight, 130·81 lbs.

23. The same parallelopipedon with an angular wedge added to its hindmost extremity:

Velocity per second, 12 f. Motive weight, 238·06 lbs.

Nautical Experiments by Col. Beaufoy.

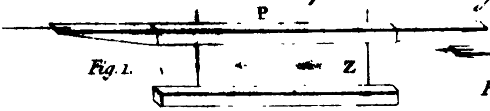

Fig. 1.

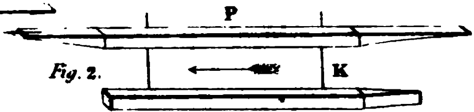

Fig. 2.

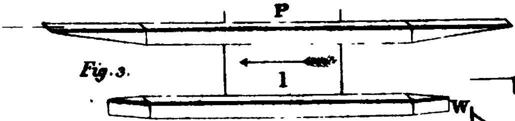

Fig. 3.

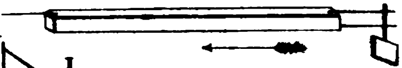

Fig. 4.

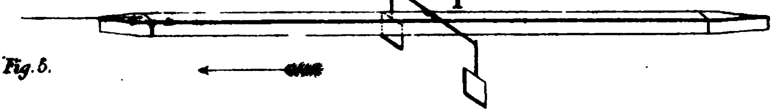

Fig. 5.

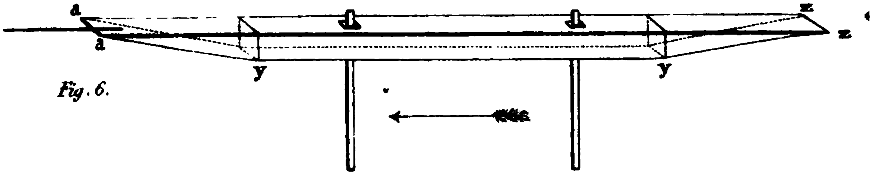

Fig. 6.

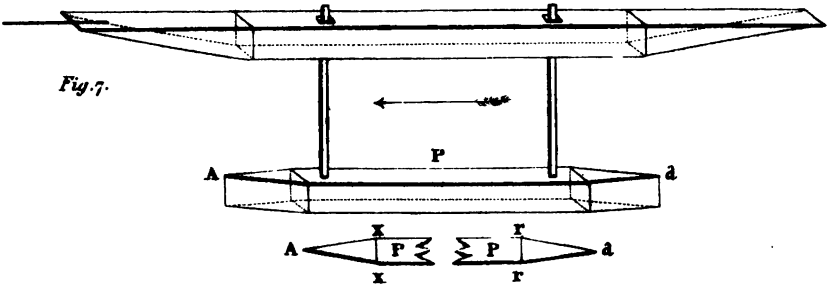

Fig. 7.

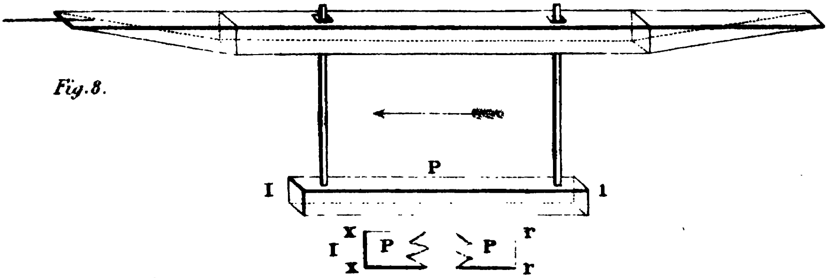

Fig. 8.

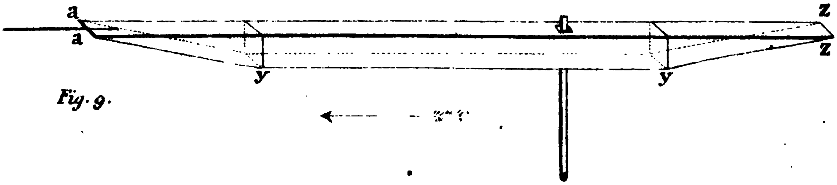

Fig. 9.

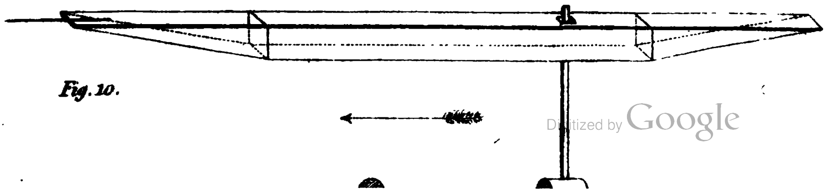

Fig. 10.

24. The same with the angular wedge foremost:

Velocity per second, 12 f. Motive weight, 115·91 lbs.

25. The parallelopipedon used in experiment 15 with a circular wedge added to each extremity:

Velocity per second, 12 f. Motive weight, 163·37 lbs.

26. The parallelopipedon of experiment 24 with an angular wedge added to each extremity:

Velocity per second, 12 f. Motive weight, 98·300 lbs.

27. The same parallelopipedon lengthened by having an inclined plane added to its hindmost extremity. The oblique side of the inclined plane is equal to the sum of the sides of the angular wedge, or 7·314 feet:

Velocity per second, 12 f. Motive weight, 223·30 lbs.

28. The same as the last, but the inclined plane foremost:

Velocity per second, 12 f. Motive weight, 108·69 lbs.

29. The same parallelopipedon with an inclined plane at each extremity:

Velocity per second, 12 f. Motive weight, 85·199 lbs.

30. The parallelopipedon of experiment 15 with an inclined plane added to each extremity:

Velocity per second, 12 f. Motive weight, 114·48 lbs.

31. An isosceles triangle with its base foremost. Length, 30·333 feet. Breadth, 3·375 feet. Depth, 1·2187 foot. Area of the base, 4·1141 feet:

Velocity per second, 6 f. Motive weight, 150·01 lbs.

32. The same triangle with its vextex foremost:

Velocity per second, 6 feet. Motive weight, 55·140 lbs.

33. An isosceles triangle with its base foremost. Length, 20·222 feet. Breadth, 3·375 feet. Depth, 1·2187 foot. Area of the base, 4·1141 feet:

Velocity per second, 6 f. Motive weight, 148·06 lbs.

34. The same triangle with its vertex foremost:

Velocity per second, 6 f. Motive weight, 49·905 lbs.

35. An isosceles triangle with its base foremost. Length, 10·111 feet. Other dimensions the same as the preceding:

Velocity per second, 6 f. Motive weight, 148·15 lbs.

36. The same triangle with its vertex foremost:

Velocity per second, 6 f. Motive weight, 49·836 lbs.

II. *Bodies Immersed under the Surface.*

37. The parallelopipedon employed in experiment 16 attached to a floating parallelopipedon, *p*, by means of two iron circular bars (see fig. 1, plate XV.), the centre of the lower body being immersed 6 feet:

Velocity per second, 12 f. Motive weight, 418·19 lbs.

38. Parallelopipedon, *p*, with the body, *k* (fig. 2), attached to it, the centre of *k* immersed 6 feet:

Velocity per second, 12 f. Motive weight, 393·11 lbs.

39. The same as the preceding, except that the pointed end of *k* is foremost:

Velocity per second, 12 f. Motive weight, 307·55 lbs.

40. The same as the preceding, except that the body *k* has now two pointed ends, and the iron bars are 9 feet 9 inches asunder:

Velocity per second, 12 f. Motive weight, 269·44 lbs.

41. Parallelopidedon, *p*, with the same parallelopipedon, *k*, immersed, lengthened by an additional wedge, *w*, at each extremity (fig. 3). Length of wedge at the foremost end, 3·6058 feet. Length of the oblique side, 3·657 feet. Length of the wedge at the hindmost part, 2·3606 feet. Length of the oblique side, 2·438 inches:

Velocity per second, 12 f. Motive weight, 293·75 lbs.

42. The same as the preceding, but the other end of the immersed body foremost:

Velocity per second, 12 f. Motive weight, 285·51 lbs.

43. The parallelopipedon, *p*, with the iron bars alone, the immersed body being removed:

Velocity per second, 12 f. Motive weight, 173·57 lbs.

44. Parallelopipedon, *p*, with a smooth pointed deal plank attached to the iron bars, its centre being immersed 6 feet. The foremost end of the deal plank was formed into the shape of an equilateral triangle. Length of plank, 20·0208 feet. Depth, 1 foot. Thickness, 3 inches. Area of the surface, 52·552 feet:

Velocity per second, 12 f. Motive weight, 226·28 lbs.

45. The same as the preceding, 1·0208 foot of the plank being left attached to the foremost bar, the rest removed:

Velocity per second, 12 f. Motive weight, 204·34 lbs.

III. *Bodies Immersed at different Depths.*

46. The parallelopipedon, *p*, lengthened by the addition of an equilateral triangle at its foremost extremity. To its aftermost extremity was fixed a round iron bar, sustaining a square iron plane, containing 2·9718 superficial feet, and the centre of the plane was immersed 3 feet (fig. 4):

Velocity per second, 12 f. Motive weight, 665·68 lbs.

47. The same as the preceding, but the centre of the plane was immersed 6 feet:

Velocity per second, 12 f. Motive weight, 722·90 lbs.

48. Same as the preceding, but the centre of the plane was immersed 9 feet:

Velocity per second, 12 f. Motive weight, 786·63 lbs.

49. The same, wanting the plane. The bottom of the round iron bar immersed, 2·3905 feet:

Velocity per second, 12 f. Motive weight, 179·56 lbs.

50. The same as the preceding, the bottom of the iron bar being immersed 5·3905 feet:

Velocity per second, 12 f. Motive weight, 223·96 lbs.

51. The same as the preceding, the bottom of the iron bar being immersed 8·3905 feet:

Velocity per second, 12 f. Motive weight, 277·49 lbs.

IV. *Immersed Bodies placed obliquely.*

52. A parallelopipedon, *i*, with two square iron planes 7 feet $10\frac{1}{2}$ inches asunder, the centre of each plane being immersed 3 feet below the surface of the water (fig. 5). Area of each plane, 2·9718 feet:

Velocity per second, 6 feet.

Angles of incidence.......	90°	80°	70°	60°	50°	40°	30°	20°	10°
Motive weight.	311·18	289·51	271·69	267·07	246	208·4	232·87	137·55	124·9 lbs.

53. Same as the preceding, but the planes were taken away and only the bars left:

Velocity per second, 6 f. Motive weight, 55 303 lbs.

54. Conductor with angular bars immersed 5·5 feet (fig. 6):

Velocity per second, 12 f. Motive weight, 84·292 lbs.

55. Conductor with parallelopipedon, P (fig. 7), A X, *r a*, each 3 feet. Length of P, 10 feet. Centre of P immersed, 6 feet:

Velocity per second, 12 f. Motive weight, 136·83 lbs.

56. Same as the preceding, except that instead of the pointed extremity, *a*, (fig. 7) the immersed body is terminated by an inclined plane, the upper surface on a level with that of the immersed body, and of the same width. Length of the inclined plane, 2 feet 10 inches. Length of the under or slanting side, 3 feet:

Velocity per second, 12 f. Motive weight, 148·77 lbs.

57. Same as in exper. 55, but the length of *r a* (fig. 7) reduced to 1·5 foot:

Velocity per second, 12 f. Motive weight, 152·30 lbs.

58. Same as the preceding, but the pointed end, *r a* (fig. 7), wanting:

Velocity per second, 12 f. Motive weight, 157·47 lbs.

59. Same as in exper. 57, but the end, *r a* (fig. 7), foremost:

Velocity per second, 12 f. Motive weight, 144·32 lbs.

60. Same as in exper 56, but the inclined plane foremost:

Velocity per second, 12 f Motive weight, 144·27 lbs.

61. Same as in exper. 59, but the foremost angular body, *r a* (fig. 7), only 1 foot in length:

Velocity per second, 12 f. Motive weight, 143·82 lbs.

62. Same as in exper. 58, but the square end of the immersed body foremost:

Velocity per second, 12 f. Motive weight, 224·60 lbs.

63. An immersed parallelopipedon 10 feet long, 1 foot broad, and 1 foot thick (fig. 8):

Velocity per second, 12 f. Motive weight, 241 65 lbs.

64. The same as the preceding; but for the immersed parallelopipedon was substituted a plank 21 feet 3 inches long, 1 foot thick, and 8 inches broad, standing in a perpendicular direction:

Velocity per second, 12 f. Motive weight, 137·41 lbs.

65. The same as the preceding, but the length of the plank reduced to 1 foot 3 inches:

Velocity per second, 12 f. Motive weight, 115·02 lbs.

66. Conductor with a round iron bar immersed 5 feet 6 inches (fig. 9). Length of *a z*, 25 feet 10 inches. Breadth, *a a* or *z z*, 1 foot. Depth, *x y*, 1 foot. Slant, *a y* or *z y*, 6 feet:

Velocity per second, 12 f. Motive weight, 90·061 lbs.

67. Same as the preceding, with a wooden triangle attached to the bottom of the round iron bar. Centre of the triangle immersed, 6 feet. Apex foremost. Base of the triangle, 1 foot. Length of a side, 3 feet. Thickness, 1 foot:

Velocity per second, 12 f. Motive weight, 137·52 lbs.

68. Same as the preceding, but the base of the triangle foremost:

Velocity per second, 12 f. Motive weight, 241·29 lbs.

69. Same as the preceding, a cube being substituted for the triangle, each face of the cube a square foot:

Velocity per second, 12 f. Motive weight, 257·70 lbs.

70. Same as the preceding, a thin square iron plane being substituted for the cube. Area of the plane, 1 foot square:

Velocity per second, 12 f. Motive weight, 247·26 lbs.

71. Same as the preceding, a round iron plane, with an area of a foot square, being substituted for the square plane. Diameter, 13·54 inches:

Velocity per second, 12 f. Motive weight, 243·97 lbs.

72. Same as the preceding, a cylinder, *u* (fig. 10), being substituted for the circular plate. Length of the cylinder, 1 foot. Area, 1 foot. Diameter, 13·54 inches:

Velocity per second, 12 f. Motive weight, 238·30 lbs.

73. Same as the preceding, with a semiglobe fixed to the hindmost end of the cylinder:

Velocity per second, 12 f. Motive weight, 219·88 lbs.

74. Same as the preceding, the semiglobe being fixed to the foremost end of the cylinder:

Velocity per second, 12 f. Motive weight, 131·70 lbs.

75. Same as the preceding, with a semiglobe at both ends of the cylinder:

Velocity per second, 12 f. Motive weight, 130·69 lbs.

76. Same as the preceding, with a globe, the diameter of which is 13·54 inches, substituted for the cylinder:

Velocity per second, 12 f. Motive weight, 140·04 lbs.

77. A new conductor, (fig. 11, plate XVI.) with a broad bar immersed 5 feet 6 inches. Length of conductor at top, *a z*, 28 feet 10 inches. Breadth at top, 3 feet 4 inches. Length at bottom, *y y*, 14 feet. Breadth at bottom, *c c*, 1 foot. Slant, *a y*, 6 feet.

Depth, *l c*, 1 foot. Depth, *x e*, 3 inches. Bar, 8 inches broad, 2 inches thick:

Velocity per second, 12 f. Motive weight, 109·31 lbs.

78. Same as the last, with a plank fixed at the bottom of the broad bar, 14 feet long, 1 foot 8 inches deep, and 3 inches thick (fig. 12, plate xvi.):

Velocity per second, 12 f. Motive weight, 170·50 lbs.

79. Same as the last, but the plank reduced in length to 2 feet:

Velocity per second, 12 f. Motive weight, 148·24 lbs.

80. Same as before, but the body, A O (fig. 13), instead of the plank. Centre of this body immersed 6 feet, x A = 3 feet, $r\ o$ = 4 feet 6 inches, $x\ r$ = 1 foot:

Velocity per second, 12 f. Motive weight, 142·42 lbs.

81. Same as the last, but the length of $r\ o$ (fig. 13) reduced to 3 feet:

Velocity per second, 12 f. Motive weight, 142·34 lbs.

82. Same as the last, but the sides $r\ o$ (fig. 13) converted into segments of a circle of 8 feet radius:

Velocity per second, 12 f. Motive weight, 142·48 lbs.

83. Same as the last, but the hinder part of A O (fig. 13) converted into a semi-ellipse:

Velocity per second, 12 f. Motive weight, 144·22 lbs.

84. Same as in exper. 81, but the length of $r\ o$ reduced to 2 feet:

Velocity per second, 12 f. Motive weight, 145·04 lbs.

85. Same as the last, but the length of $r\ o$ (fig. 13) reduced to 1 foot 6 inches:

Velocity per second, 12 f. Motive weight, 149·20 lbs.

86. Same as the last, but the length of $r\ o$ (fig. 13) reduced to 1 foot:

Velocity per second, 12 f. Motive weight, 166·41 lbs.

87. Same as the last, but $r\ o$ (fig. 13) converted into segments of circles of 1 foot radius:

Velocity per second, 12 f. Motive weight, 155·29 lbs.

88. Same as the last, but the hinder part of the immersed body converted into a semicylinder of 6 inches radius (fig. 14):

Velocity per second, 12 f. Motive weight, 152·12 lbs.

89. Same as the last, but the hinder part of the immersed body wanting, it terminates at $r\ r$ (fig. 13):

Velocity per second, 12 f. Motive weight, 157·93 lbs.

90. Same as in exper. 82, but the end of the immersed body formed of segments of circles foremost:

Velocity per second, 12 f. Motive weight, 141·62 lbs.

91. Same as in exper. 83, but the semi-elliptical end of the immersed body foremost:

Velocity per second, 12 f. Motive weight, 142 lbs.

92. Same as in exper. 84, but the shortest end of the immersed body foremost:

Velocity per second, 12 f. Motive weight, 142·66 lbs.

93. Same as in exper. 85, but the shortest end of the immersed body foremost:

Velocity per second, 12 f. Motive weight, 144·56 lbs.

94. Same as in exper. 86, the shortest end of the immersed body foremost:

Velocity per second, 12 f. Motive weight, 152·14 lbs.

95. Same as in exper. 87, but the circular end of the immersed body foremost:

Velocity per second, 12 f. Motive weight, 141·36 lbs.

96. Same as in exper. 88, but the cylindrical end of the immersed body foremost:

Velocity per second, 12 f. Motive weight, 146·65 lbs.

97. Same as the last, but the hindermost extremity of the immersed body, *x* A, (fig. 14) 4 feet 6 inches in length:

Velocity per second, 12 f. Motive weight, 146·52 lbs.

98. Same as in exper. 89, but the blunt end of the immersed body foremost:

Velocity per second, 12 f. Motive weight, 232·83 lbs.

99. Same as the last, but the hindermost and pointed end, *x* A (fig. 14) of the immersed body 4 feet 6 inches in length:

Velocity per second, 12 f. Motive weight, 232·47 lbs.

100. Usual floating conductor. Immersed body, a cube of 1 foot, with a semicylinder placed before it of 6 inches radius (fig. 15):

Velocity per second, 12 f. Motive weight, 159·39 lbs.

101. Same as the last, the cylindrical end of the immersed body hindermost:

Velocity per second, 12 f. Motive weight, 248·21 lbs.

102. Same as the last, but a semicylinder at each end of the immersed body:

Velocity per second, 12 f. Motive weight, 156·92 lbs.

103. Same floating conductor. A cylinder, 1 foot in length; and the area of its circular extremities, 1 foot. Diameter, 13·54 inches:

Velocity per second, 12 f. Motive weight, 260·27 lbs.

104. Same as the last, with a semisphere attached to the fore end of the cylinder:

Velocity per second, 12 f. Motive weight, 150·76 lbs.

105. Same as the last, but the hemisphere behind:

Velocity per second, 12 f. Motive weight, 240·80 lbs.

106. Same as the last, with a hemisphere at both ends of the cylinder:

Velocity per second, 12 f. Motive weight, 146·33 lbs.

ARTICLE V.

Essay on the Cause of Chemical Proportions, and on some Circumstances relating to them: together with a short and easy Method of expressing them. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

(*Continued from* Vol. II. p. 451.)

III. *On the Chemical Signs, and the Method of employing them to express Chemical Proportions.*

WHEN we endeavour to express chemical proportions, we find the necessity of chemical signs. Chemistry has always possessed them, though hitherto they have been of very little utility. They owed their origin, no doubt, to the mysterious relation supposed by the alchymists to exist between the metals and the planets, and to the desire which they had of expressing themselves in a manner incomprehensible to the public. The fellow-labourers in the antiphlogistic revolution published new signs founded on a reasonable principle, the object of which was, that the signs, like the new names, should be definitions of the composition of the substances, and that they should be more easily written than the names of the substances themselves. But, though we must acknowledge that these signs were very well contrived, and very ingenious, they were of no use; because it is easier to write an abbreviated word than to draw a figure, which has but little analogy with letters, and which, to be legible, must be made of a larger size than our ordinary writing. In proposing new chemical signs, I shall endeavour to avoid the inconveniences which rendered the old ones of little utility. I must observe here that the object of the new signs is not that, like the old ones, they should be employed to label vessels in the laboratory: they are destined solely to facilitate the expression of chemical proportions, and to enable us to indicate, without long periphrases, the relative number of volumes of the different constituents contained in each compound body. By determining the weight of the elementary volumes, these figures will enable us to express the numeric result of an analysis as simply, and in a manner as easily remembered, as the algebraic formulas in mechanical philosophy.

The chemical signs ought to be letters, for the greater facility of writing, and not to disfigure a printed book. Though this last circumstance may not appear of any great importance, it ought to be avoided whenever it can be done. I shall take, therefore, for the chemical sign, the *initial letter of the Latin name of each elementary substance:* but as several have the same initial letter, I shall distinguish them in the following manner:—1. In the class which I call *metalloids,* I shall employ the initial letter only, even when this letter is common to the metalloid and to some metal. 2. In the class of metals, I shall distinguish those that have the same initials with another metal, or a metalloid, by writing the first two letters of the word. 3. If the first two letters be common to two metals, I shall, in that case, add to the initial letter the first consonant which they have not in common: for example, S = sulphur, Si = silicium, St = stibium (antimony), Sn = stannum (tin), C = carbonicum, Co = cobaltum (cobalt), Cu = cuprum (copper), O = oxygen, Os = osmium, &c.

The chemical sign expresses always one volume of the substance. When it is necessary to indicate several volumes, it is done by adding the number of volumes: for example, the *oxidum cuprosum* (protoxide of copper) is composed of a volume of oxygen and a volume of metal; therefore its sign is $Cu + O$. The *oxidum cupricum* (peroxide of copper) is composed of 1 volume of metal and 2 volumes of oxygen; therefore its sign is $Cu + 2\,O$. In like manner, the sign for sulphuric acid is $S + 3\,O$; for carbonic acid, $C + 2\,O$; for water, $2\,H + O$, &c.

When we express a compound volume of the first order, we throw away the +, and place the number of volumes above the letter: for example, $Cu\,O + S\,\overset{3}{O}$ = sulphate of copper, $Cu\,\overset{2}{O} + 2\,S\,\overset{3}{O}$ = persulphate of copper. These formulas have this advantage, that if we take away the oxygen we see at once the ratio between the combustible radicles. As to the volumes of the second order, it is but rarely of any advantage to express them by formulas as one volume; but if we wish to express them in that way, we may do it by using the parenthesis, as is done in algebraic formulas: for example, alum is composed of 3 volumes of sulphate of alumina and 1 volume of sulphate of potash. Its symbol is $3\,(Al\,\overset{2}{O} + 2\,S\,\overset{3}{O}) + (\overset{2}{Po} + 2\,S\,\overset{3}{O})$. As to the organic volumes, it is at present very uncertain how far figures can be successfully employed to express their composition. We shall have occasion only in the following pages to express the volume of ammonia. It is $6\,H + N + O$, or $\overset{6}{H}\,N\,O$.

IV. *Weight of elementary Volumes compared with that of Oxygen Gas.*

A.—*Oxygen.*

The volume of oxygen is expressed by the letter O. It is considered = 100.

B—*The Metalloids.*

1. *Sulphuricum, sulphur* (S).—I have already mentioned that we may determine the volume of this body by the quantity of sulphur which combines with a given weight of metal compared with the oxygen which combines with the same metal. It is to be supposed that the relative quantities of sulphur and oxygen have the same

volume, because this relation between them is constant, not only with respect to all the metals, but likewise with respect to carbon and hydrogen. As 100 parts of lead combine with 7·7 parts of oxygen and with 15·42 parts of sulphur, the volume of the first ought to be to that of the last as 77 : 154·2, or 100 : 201. But, on the other hand, if we take for the base of our calculation an analysis of sulphate of lead, which I made by neutralizing a determinate quantity of oxide of lead with sulphuric acid (Ann. de Chim. Aug. 1811, p. 121), we find the volume of sulphur to weigh as much as 210. On the other hand, if we oxidate lead by means of nitric acid, and pour sulphuric acid into the solution, we obtain, after evaporation and ignition of the mass, a quantity of sulphate of lead (Ann. de Chim. Oct. 1811, p. 11), which, taken as the base of our calculation, gives us the volume of sulphur as light as 200. There is a want of precision in some of these experiments, which makes the weight of a volume of sulphur uncertain between 200 and 210.

We know that sulphur combines with oxymuriatic gas in two proportions, constituting two muriates with different bases. In one of these muriates the sulphur is combined with $\frac{1}{2}$ as much oxygen as in sulphurous acid, and in the other with $\frac{1}{4}$ as much. Now is it not probable that these degrees of oxidation are $S + O$, $S + 2\,O$, $S + 4\,O$, $S + 6\,O$? In which case the acid is composed of 1 volume of sulphur and 6 volumes of oxygen. The following circumstances strengthen this opinion:—1. Most acids in *ic* contain 6 volumes of oxygen, while the acids in *ous* contain 4 volumes. The only decided exceptions are the phosphoric and muriatic acids. In the neutral chromates and arseniates the acid contains three times as much oxygen as the base, though these acids contain evidently 6 volumes of oxygen. 2. If we suppose sulphuric acid to be $S + 3\,O$, the persubsulphate of copper (*subsulphas cupricus*) is $1\frac{1}{2}\,Cu\,\overset{2}{\dot{O}} + S\,\overset{3}{\dot{O}}$; but if we suppose this acid to be $S + 6\,O$, the salt in question will be $3\,Cu\,\overset{2}{\dot{O}} + S\,\overset{6}{\dot{O}}$; and the anomaly of the half volume would be destroyed. These observations give some probability to the notion; yet the constant relation between the weight of sulphur and oxygen which unite most readily with any metal, added to the circumstance that no compound equivalent to $S + 3\,O$ (supposing sulphuric acid $S + 6\,O$) has been yet found, though such a compound ought not only to exist, but to combine in preference with bases—these circumstances seem sufficiently to refute the notion, and to prove that the four degrees of oxidation of sulphur ought to be expressed thus: $2\,S + O$, $S + O$, $S + 2\,O$, $S + 3\,O$. The argument drawn from the persubsulphate of copper loses much of its value when we compare it with the *subarseniates*, in which half a volume of the base is added to the neutral arseniates, and in which no explanation will make this half disappear.

2. *Muriaticum, muriatic radicle* (M).—Though we are unable to obtain this body in a separate state, or to combine it with any

other body except oxygen, we are able, however, from the laws of chemical proportions, to determine the weight of its volume: for if we compare the different degrees of oxidation of muriatic acid with the composition of the muriates, we find that muriatic acid must be M + 2 O. If, according to one of my experiments (Ann. de Chim. Aug. 1811, p. 153), 100 parts of silver unite with 7·44 parts of oxygen; and if 100 parts of muriate of silver be composed of 19·035 of muriatic acid and 80·965 of oxide of silver, then the volume of muriatic radicle will weigh 139·56: but, on account of our ignorance of the true weight of a volume of sulphur, it is possible that 100 parts of silver, when oxidated, absorb only 7·36 parts of oxygen. In that case the muriate of silver will contain 19·091 of muriatic acid, and the volume of muriatic radicle will weigh as much as 162·2. From this it is obvious how very accurately the muriate must be analyzed to obtain a result by calculation which may be depended on. It follows from the determination which I have just stated, that the degrees of oxidation of the muriatic radicle at present known, are, muriatic acid, M + 2 O; oxymuriatic gas (*hyperoxydum muriatosum*), M + 3 O; euchlorine gas (*hyperoxydum muriaticum*), M + 4 O; oxymuriatic acid, M + 8 O. Is there a combination M + 6 O? and is it an *oxymuriatous* acid? We know that a solution of caustic potash impregnated with oxymuriatic gas has very different properties from those of a solution of hyper-oxymuriate of potash. Does it owe its peculiar properties, as those of bleaching, to the presence of an hyper-oxymuriate of potash which is decomposed into muriate and oxymuriate by the act of crystallization?

3. *Phosphoricum, phosphorus* (P).—In an analysis of phosphate of lead (Ann. de Chim. Oct. 1811, p. 6) I found that 100 parts of phosphoric acid are neutralized by 380·56 parts of oxide of lead. M. Rose found that 100 parts of phosphorus absorb, in order to be converted into an acid, 111 parts of oxygen. This proves that the 100 parts of phosphoric acid cannot contain more than two times as much oxygen as the bases which neutralize it. If we consider that the analysis of the phosphate of lead is susceptible of greater exactness than an experiment to unite the constituents of the acid can be, it will follow that a volume of phosphorus ought to weigh 167·512. If we make the same calculation from my analysis of the phosphate of barytes, we obtain for the volume of phosphorus 167·3. This determination is founded on the supposition that phosphoric acid is P + 2 O. At the same time it must be allowed to be unlikely that phosphorous acid should contain only a single volume of oxygen. Davy establishes (Elements of Chemical Philosophy, p. 289,) that phosphorus in phosphorous acid is combined with half the quantity of oxygen with which it is combined in phosphoric acid. But as he found that 100 parts of phosphorus, in order to become phosphorous acid, absorb 77 parts of oxygen, one would be rather disposed to conclude that the oxygen in phosphorous acid is to that in phosphoric acid as 2 : 3. I have never made any

experiments, either with phosphorous acid, or with the white or red oxides of phosphorus; but I should be disposed to consider the white oxide as P + O, phosphorous acid as P + 2 O, and phosphoric acid as P + 4 O. The nature of the red oxide is still doubtful, in consequence of the opposite conclusions drawn by Thenard on the one hand, and Vogel and Seeback on the other, from their experiments.

4. *Fluoricum, fluoric radicle* (F).—The volume of fluoric radicle might be calculated in the same way as that of the muriatic, if we had exact analyses of a sufficient number of fluates. Different chemists have analysed native fluate of lime, and have obtained results too variable to put any confidence in them. Wenzel found it a compound of fluate of lime and fluate of alumina. According to him, 100 parts of fluoric acid neutralize 202 parts of lime. Richter found that 100 parts of fluate of lime produce 147·3 parts of sulphate of lime. Thomson obtained 156·6 parts of sulphate of lime, and Klaproth extracted 123¼ parts of carbonate of lime. If we calculate the quantity of lime contained in the sulphate and carbonate, we obtain the proportion which exists in the fluate. Mr. Dalton, in an analysis of which I do not know the details, finds still less lime in this fluate than the other chemists. According to these experiments, 100 parts of fluoric acid ought to combine with 200 parts of lime according to Wenzel, with 160 according to Richter, with 191·58 according to Thomson, with 228 according to Klaproth, and with 150 according to Dalton. These differences announce that the fluate of lime is not always of the same nature. It probably always contains a portion of triple fluate of lime-and-silica, which occasions this great diversity in the analytical results. To discover the capacity of saturation of fluoric acid, it is necessary to examine an artificial combination absolutely pure: for example, fluate of barytes and fluate of silver. Mr. John Davy has examined the gaseous fluate of silica; and if the composition of silica were known as exactly as that of lime, it would be easy to determine the constitution of the acid from that analysis. When I come to speak of silica, I shall have occasion to state more at large the composition of that earth.

All these experiments show that fluoric acid neutralizes such a quantity of base that the acid can only contain a quantity of oxygen equal to that which exists in the base. If we take the analysis of fluate of lime by Thomson as the base of our calculation, the acid ought to contain about 55 per cent. of oxygen; but if we employ the analysis of silicated fluoric acid made by Mr. John Davy, the acid ought to contain from 76 to 77·5 per cent. of oxygen. In order to determine how many volumes these 77 per cent. amount to, we may employ an experiment of Gay-Lussac, confirmed by Mr. John Davy, according to which 1 volume of silicated fluoric acid condenses 2 volumes of ammoniacal gas. The fluoric acid and silica, from what has just been said, ought to contain equal volumes of oxygen; but ammonia contains the fourth part of its volume of oxygen; and the 2 volumes of ammoniacal gas contain half as much oxygen in weight as the quantity of silica which exists in a volume of silicated fluoric acid. Hence fluoric acid must contain likewise twice as much oxygen as ammonia. Hence it follows that, removing the silica, the fluate of ammonia remaining is composed in such a manner that the acid contains twice as much oxygen as the base.

When we compare the analysis of the gaseous silicated fluoric acid with that of the subfluate of ammonia formed when we separate the silica from the fluate of ammonia-and-silica, we find that the ammonia which comes in place of the silica contains 1½ times as much oxygen as the silica;* and consequently in the subfluate of ammonia the base contains twice as much oxygen as the acid; that is to say, that the acid is combined with four times as much ammonia as in the triple salt. I request the reader who wishes to follow these calculations to compare what I have said here with the interesting memoir of Mr. John Davy in the Philosophical Transactions for 1812, and likewise with what I say in the sequel when speaking of silica.

As there is such a correspondence in the experiments of Mr. John Davy, I consider it as most probable that fluoric acid contains nearly 77 parts of oxygen and 23 parts of radicle, and that these 77 parts are 2 volumes. Hence the volume of fluoric radicle ought to weigh almost exactly as 60. If the analysis of fluate of lime by Dr. Thomson were most exact, the radicle would weigh as high as 80.

5. *Boracicum, boron* (B).—We know, from the ingenious experiments of Sir Humphry Davy, as well as from those of Thenard and Gay-Lussac, the nature of this body in a separate state. Davy found boracic acid to contain 73 per cent. of oxygen, while Thenard and Gay-Lussac affirm that it contains only the third of its weight of that principle. To determine the composition of this acid, I have examined some of its combinations, in order to ascertain its capacity for saturation.

1. *Boracic acid and water.*—(*a.*) A portion of vitrious and very pure boracic acid was dissolved in boiling water, and then crystallized. The crystals were dried, reduced to powder, and exposed upon paper for 24 hours to the temperature of 68°. The acid thus dried was

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* Silica is supposed to be composed of 48 oxygen and 52 silicum. The fluate of ammonia-and-silica is composed in such a manner that the oxygen of the ammonia being 1, that of the silica and of the fluoric acid is each 2. Now if the ammonia replaced the silica in such a manner as to contain a quantity of oxygen equal to that in the silica disengaged, the oxygen of the whole ammonia ought to be equal to the oxygen of the silica and to the oxygen of the ammonia in the triple salt; that is to say, that the oxygen in the base ought to be to that in the acid as 3 : 2. But as this is contrary to the laws of chemical proportions, it is necessary that the ammonia which takes the place of the silica should contain either ½ the oxygen in the silica, or 1½ the oxygen; that is to say, that in the neutral fluate of ammonia the ammonia ought to contain a quantity of oxygen equal to that in the acid, or twice as much.

put into a glass capsule, and exposed on a sand-bath to a heat considerably exceeding that of boiling water. It lost 22·1 per cent. of its weight. Being now heated in a platinum crucible by the flame of a spirit lamp, it lost still 12·9 per cent., making the whole loss 35 per cent. The lid of the crucible exhibited traces of sublimed boracic acid.—(*b*.) I mixed 10 parts of dry boracic acid in powder with 40 parts of oxide of lead, which had been heated to redness immediately before. On this mixture I poured water in a platinum crucible. I evaporated this water slowly to dryness, and repeated this process three times. The mass, after being dried the fourth time, was exposed to a heat sufficient to melt it. The glass thus formed weighed 45·6 parts. Hence the 40 parts of oxide of lead had combined with 5·6 parts of boracic acid, and 4·4 parts of water had been driven off. This is exactly twice as much as the boracic acid lost by exposure on the sand-bath. These experiments seem to prove that boracic acid contains 2 proportions of water, 1 of which is water of crystallization, while the other is a base to the acid. In a moderate heat it loses its water of crystallization, but the other portion remains. It appears that by a still stronger heat half the water which acts as a base is disengaged, leaving for residue a *superboras hydricus,* which is entirely decomposed in a red heat.

2. *Borate of Ammonia.*—Ten parts of borate of ammonia, crystallized and very pure, were dried, and put in the state of powder into a small retort with four times its weight of pure lime. I adapted to the retort a small tubulated receiver filled with caustic potash, and furnished with a glass tube to allow the ammoniacal gas to escape. This tube, as well as the receiver, was filled with caustic potash, and the whole was exactly weighed. I now heated the retort till the whole of its contents was red-hot, and till the disengagement of ammoniacal gas had entirely ceased. The receiver and tube had gained 3·173 parts of water, and the crucible had lost 6·205 parts of its weight. Hence it follows that the lime had retained 3·795 parts of boracic acid, and had disengaged 3·032 parts of ammoniacal gas: therefore borate of ammonia is composed of

Boracic acid	37·95
Ammonia	30·32
Water of crystallization	31·73
	100·00

Now 30·32 of ammonia contain 13·886 of oxygen, and 31·73 of water contain 28 parts: but $13·886 \times 2 = 27·772$; so that the water of crystallization constitutes 2 proportions for 1 of ammonia. According to the result of this experiment, 100 parts of acid are combined with 79·895 parts of ammonia: and if, as I have shown in a former essay, ammonia contains 45·8 per cent. of oxygen, these 79·895 contain 36·59 parts. Boracic acid, then, cannot contain more than twice as much oxygen as the base by which it is neutralized. Hence it ought to contain 73·18 per cent. of oxygen, which coincides sufficiently with the experiment of Davy. If, on the other hand, we calculate the composition of boracid acid from the quantity of water contained in the crystallized acid, the proportion of oxygen will amount only to 69·4 per cent.

3. I endeavoured to verify the preceding determination by other analyses; as for example, by analyzing borate of lead and borate of barytes; but I did not obtain satisfactory results. Borate of lead yielded, in different experiments, from 116 to 118 per cent of sulphate of lead; but the liquid from which this had been precipitated still contained lead, and I was not able to determine exactly how much. The result was still less satisfactory when I precipitated a given weight of nitrate of lead by borate of ammonia, because the borate of lead formed is soluble in the water employed to wash it. Borate of barytes presents difficulties still greater, because boracic acid forms several compounds with barytes, all of which contain more acid in proportion than the alkaline borates by means of which they are formed. These borates are more or less soluble in water, and in these solutions carbonic acid decomposes them, precipitating carbonate of barytes. Borate of barytes formed by precipitating muriate of barytes by means of borate of ammonia, and well washed, was deprived of its combined water by exposure to heat: 100 parts of this borate dissolved in nitric acid, and decomposed by sulphuric acid, produced 63·92 parts of sulphate of barytes, equivalent to 41·93 of barytes; that is to say, that 100 parts of boracic acid had been combined with 72·2 parts of barytes. If we consider the borate of ammonia as neutral, this borate of barytes is a superborate in which the acid contains 10 times as much oxygen as the base; that is to say, in which the base is combined with five times as much acid as in the neutral borate. By analysing in the same way the precipitate obtained from muriate of barytes by common borax, I obtained from 100 parts of calcined borate of barytes 85 parts of sulphate of barytes. In this borate 100 parts of acid are united with 126 parts of barytes; that is to say, that the acid ought to contain six times as much oxygen as the base. The base, of course, is combined with three times as much acid as in the neutral borate. These two borates are soluble in water; but the second is much more so than the first. Boiling water dissolves little more than cold water; and the small surplus falls in the state of a white powder without any appearance of crystallization: even when the liquid is evaporated no crystals are formed; and if the evaporation be performed in an open vessel, flocks fall which consist partly of carbonate, partly of borate of barytes. I thought it worth while to relate these experiments, in order to show the difficulty of controlling the result of the analysis of borate of ammonia, and to make known some of these borates, hitherto scarcely examined.

From the preceding experiments, I conceive we may conclude that boracic acid contains 73·18 per cent. of oxygen, and that these 73·18 constitute two volumes. The acid, then, is B + 2 O; and a volume of radicle must weigh 73·273.

6. *Carbonicum, carbon* (C).—M. Biot has ascertained by delicate and exact experiments that the specific gravity of oxygen, compared with that of atmospherical air, is 1·10359 to 1; and that the weight of the same volume of carbonic acid gas is 1·51961. Hence it follows that carbonic acid is composed of 72·62 of oxygen and 27·38 of carbon. M. de Saussure has determined the weight of oxygen gas 1·10562, from which it follows that carbonic acid ought to contain 72·75 of oxygen; and, finally, Saussure found, by a direct experiment, that when very pure charcoal, obtained by exposing oil of rosemary to a strong heat, is employed, 27·12 parts of charcoal form 100 of carbonic acid, which ought, therefore, to contain 72·88 of oxygen. On the other hand, we know already that in the carbonates the acid contains either twice or four times as much acid as the base. From this it follows that carbonic acid ought to contain either 2 or 4 volumes of oxygen. The circumstance that carbonic acid gas contains exactly four times as much oxygen as the same bulk of ammonia, appears very favourable to the opinion that carbonic acid contains four volumes of oxygen. But there are other considerations which render that idea less probable. We know, for example, that oxygen gas, in combining with the quantity of carbon requisite to produce carbonic oxide, doubles exactly its ordinary volume. It appears here reasonable to think that the additional volume is carbon; for it is contrary to the experiments hitherto made to suppose that oxygen gas in combining with half its volume of carbon should experience an expansion equal to the half of its own volume. We know several examples where two gases combine without undergoing any contraction in their volume: we know likewise a great many instances where two gases in combining contract half their volume, or even more; but, as far as I know, we are not acquainted with any example of two gaseous bodies dilating when they combine. Hence we must conclude that carbonic oxide is C + O, and carbonic acid C + 2 O. Besides, if we consider the imperfectly acid properties of carbonic acid, it is reasonable to consider as supercarbonates those combinations in which the acid contains four times as much oxygen as the base; and as neutral carbonates, those in which it contains twice as much oxygen as the base, as the carbonates of lime, barytes, lead, &c. (I refer here to what I have already said on this subject in the *Ann. de Chim.* Sept. 1811, p. 264.)

Although the determinations of the composition of this acid stated above must be very exact, it appeared to me that a verification by means of an analysis of carbonate of lead, would be very interesting. I found, however, that the analysis of this salt, though extremely simple, is attended with difficulties which render it less exact than the determinations above stated. These difficulties are owing to the great readiness with which carbonate of lead combines with organic or volatile matter, from which it is not easy to get water absolutely free: even the alkaline carbonates employed to precipitate it contain such matters. The consequence has been, that I was unable to obtain carbonate of lead whose carbonic acid, when distilled, had not an empyreumatic odour. I shall state, however, those experiments which were the most successful.

I dissolved pure nitrate of lead in water distilled over again in a glass alembic, and I divided this solution into two parts, one of which was precipitated by the carbonate of ammonia, the other by the carbonate of soda. I found it necessary to add an excess of alkaline carbonate, and to digest the precipitate in this excess, otherwise a portion of subnitrate of lead at a maximum is always formed, and the precipitate when decomposed by heat yields towards the end of the process red vapours of nitrous acid gas, in quantity sufficient to affect the result. The carbonate of ammonia employed had been prepared from the purest, most colourless, and transparent sal-ammoniac that I could procure by distilling it with pure carbonate of lime heated to redness just before it was mixed with the sal-ammoniac. In one of these experiments I had sublimed the sal-ammoniac in a glass vessel before mixing it with the carbonate of lime: but, notwithstanding all these precautions, the carbonate of lead obtained by means of this carbonate of ammonia gave out a carbonic acid when heated, which smelled distinctly of the oil of hartshorn. A very small quantity, however, of this oil must have been sufficient to produce the smell: for the result of the analysis by means of carbonate of ammonia differs very little from that by means of carbonate of soda. The carbonate of soda employed was prepared from a very pure supertartrate of soda. Hence it contained neither potash, nor sulphate, nor muriate of soda. The carbonate of lead obtained by means of carbonate of soda yielded a carbonic acid which had less odour than the other, but which was, notwithstanding, mixed with something empyreumatic.

To analyse these two carbonates I dried them thoroughly on a sand-bath in a heat much above 212°. I then put them into small retorts, which were exactly weighed, and which terminated in glass tubes filled with pieces of melted muriate of lime. These tubes were likewise carefully weighed before the experiment. The retorts were then placed upon the fire, and heated till the oxide of lead melted. From these two precipitates I obtained the following results:—

	Precipitate by carbonate of soda.	Ditto by carbonate of ammonia.
Carbonic acid	16·442	16·447
Oxide of lead	83·333	83·333
Moisture	0·225	0·220
	100·000	100·000

The difference between these two results is too small to merit any attention; yet the empyreumatic odour of the disengaged gas shows that they are not absolutely exact. If we take them for the base of our calculation, we find that 100 of carbonic acid combines with 506·83 of oxide of lead, which oxide contains 36·24 parts of oxygen: but 36·24 × 2 = 72·43. This approaches very near the determinations given above.

M. Chevreul has lately found, by repeating some of my experiments on the salts of lead, that carbonate of lead leaves for residue 83·65 parts of oxide of lead. Hence it follows that, neglecting the humidity, which is very trifling, it contains 16·35 parts of carbonic acid: but as M. Chevreul prepared his carbonate of lead by passing a current of carbonic acid gas through the solution of subnitrate of lead, it is probable that it contained some subnitrate of lead at a maximum; at least I have found this to be the case. Even when I decomposed in the same manner a solution of subacetate of lead, the precipitate, though well washed with boiling water, contained a portion of subacetate of lead at a maximum sufficiently great to reduce a part of the oxide to the metallic state when I decomposed the carbonate by heat.

If we adopt as most exact the composition of carbonic acid as determined by the specific gravity of the gases ascertained by Biot, the volume of carbon will weigh 75·4: if we adopt Saussure's experiments, it will weigh 74·914; if mine, 75·9; if that of Chevreul, 73·6

7. *Nitricum, radicle of azote* (N).—I have proved in a former paper that nitric acid is composed of 11·71 of nitric radicle and 88·29 of oxygen; and that nitric acid is N + 6 O. Hence the volume of *nitricum* must weigh 79·451. If we calculate the weight of nitricum from the weight of oxygen and azotic gases, the last of which contains half its volume of oxygen gas, it will be found to weigh only 75·5. It is difficult to explain the cause of this difference: for very little variation in the base of these calculations produces a great difference in the result. If, for example, in calculating the specific gravity of azotic gas from the specific gravity of oxygen gas and atmospherical air, we make the volume of azote in air too small, the calculation will give the weight of nitricum too small. On the other hand, if 100 parts of nitrate of lead were to give 67·4 parts of oxide of lead, instead of 67·3 or 67·31, the volume of *nitricum* would be exactly 75·5. Perhaps both methods are to a certain amount inaccurate.

I have endeavoured to prove that azote, or the sub-oxide of nitricum, is composed of N + O; nitrous oxide, of N + 2 O; nitric oxide, or nitrous gas, of N + 3 O; nitrous acid, of N + 4 O; and nitric acid, of N + 6 O. The oxides of nitricum are the first series of oxides of which we know all the degrees. The radicle of ammonia, as I have shewn in a preceding dissertation, is composed of N + 6 H. May it be considered as a compound metal?

8. *Hydrogenium, hydrogen* (H).—We know that water is composed of 2 volumes of hydrogen and 1 volume of oxygen. According to the specific gravity of these gases determined by Biot, the

volume of hydrogen weighs 6·686. If, on the other hand, the determination of Gay Lussac (*Mem d' Arcueil,* ii. 285) be not an error of the press, it would make the volume of hydrogen to weigh 7·65. It is only in organic nature that we become acquainted with other degrees of oxidation of hydrogen besides water. In ammonia 6 volumes of hydrogen are combined with 1 volume of oxygen: in oxalic acid 1 volume of hydrogen is combined with 18 volumes of oxygen.

(To be continued.)

ARTICLE VI.

On the Structure of Crystals from Spherical Particles of Matter.

By Mr. N. I. Larkin.

(To Dr. Thomson.)

SIR,

THE handsome manner in which you introduced my former communication encourages me to trouble you with a few properties I have since discovered peculiar to the tetrahedral pile of spheres.

That a rhomboidal dodecahedron may be formed from a cube by application of quadrangular pyramids to each face is known. I attempted to construct it of spheres on that principle, and found that a cube of three spheres on each edge was capable of being converted into a dodecahedron by applying at each face a quadrangular pyramid of five spheres; so that each of the four spheres composing the base may stand against the side of the cube instead of being opposite to the angles: the result was unexpected, for it proved, when completed, to be an octohedron with a single sphere on each face, agreeing in that general property with the cube described in my former paper: the difference is in the dimensions of the octohedrons. In the cube the octohedron has two spheres on each edge: in the rhomboidal dodecahedron it has five.

I have likewise succeded in forming the twenty-four sided figure common to the white garnet, from a rhomboidal dodecahedron, which was constructed from an octohedron having thirteen spheres on each edge; it proved to be a canted cube with a single sphere on each of the triangular faces, and a peculiar kind of quadrangular pyramid on each of its square faces.

The following properties of those figures in their first rudiments, which have been hitherto formed of spheres, according to the tetrahedral structure, may be worth reciting. The tetrahedron, octohedron, and rhomboid, are the most simple, having no other spheres than those which form their solid angles; the first two are the only ones that can properly be called simple, and have nothing apparently in common but their faces; the third may be considered as two of the first joined base to base, or as one of the second with a sphere on each of two opposite faces. The next in order is the cube, which is two tetrahedrons, whose solid angles mutually protrude through each other's faces, having an octohedron in their centres common to both; or it may be considered as an octohedron or rhomboid with a sphere on each face: the rhomboidal dodecahedron partakes of a degree of simplicity with the above figures, in having its surface marked out by a point at each solid angle, and having none but them to designate its edges: the last figure I have discovered deviates from the above simplicity, in having some intermediate spheres between its solid angles to mark its edges—it may be remarked that every sphere that is completely enveloped in this structure is surrounded by twelve in the form of a canted cube.

The formulas for ascertaining the number of spheres necessary to make any of the above figures may be serviceable: let the number of spheres on the edge of the required figure $= n$; then the tetrahedron $= \frac{n^3 + 3n^2 + 2n}{6}$, the octohedron $= \frac{\overline{n^2 + n} \times \overline{2n + 1}}{3} - n^2$, the rhomboid $= n^3$, the cube $= n^3 + \overline{n - 1}|^2 \times 3n$. Let n represent the number of spheres on the edge of the cube in what follows; then the number of spheres on the edge of the largest tetrahedron contained in the cube $= 2n - 1$; the number on the edge of the contained octohedron or rhomboid $= n$, being always the same as the cube. Hence the contained rhomboid is $= n^3$; and the remaining spheres necessary to make the rhomboid a cube will be $= \overline{n - 1}|^2 \times 3n$, which may be considered a square prism.

The relation between the rhomboid and the cube shows that the Equiaxe of Haüy might be easily constructed from a rhomboid formed of oblate spheroids, similar to that described by Dr. Wollaston in the Bakerian Lecture, 1813.

In hopes that the above remarks may eventually prove useful to crystallography, your insertion of them in the *Annals of Philosophy* will greatly oblige,

Sir, your most obedient servant,

Somers' Town, London, Dec. 14, 1813. N. I. LARKINS.

ARTICLE VII.

Astronomical and Magnetical Observations at Hackney Wick.

By Col. Beaufoy.

Latitude, 51° 32′ 40″ North. Longitude West in Time 6″$\frac{82}{100}$.

Immersion of δ ♑ ..... $\left\{\begin{array}{llll} 5^h & 50' & 57'' & \text{Mean Time.} \\ 6 & 02 & 20 & \text{Apparent Time.} \end{array}\right.$

The unilluminated part of the moon's disk was well defined, the disappearance of the star instantaneous, and it retained its brightness without any sensible diminution till the immersion.

Magnetical Observations.

1813.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Nov. 18	8h 50'	24° 17' 10"	—h —'	—° —' —"		Discontinued, from the shortness of the days.
Ditto 20	8 30	24 15 54	— —	— — —		
Ditto 21	8 45	24 15 00	1 55	24 20 48		
Ditto 22	— —	— — —	1 55	24 19 25		
Ditto 23	8 45	24 18 00	1 55	24 20 58		
Ditto 24	8 45	24 16 42	1 55	24 20 25		
Ditto 25	8 57	24 17 54	2 05	24 20 14		
Ditto 26	9 00	24 16 43	2 05	24 19 14		
Ditto 27	8 45	24 17 14	1 55	24 19 22		
Ditto 28	8 45	24 17 34	1 47	24 19 58		
Ditto 29	8 45	24 18 48	1 52	24 17 04		
Ditto 30	8 30	24 16 43	1 55	24 18 37		

Mean of Observations in Nov.	Morning	at	8h 42'	..... Variation	24° 17' 42"	West.
	Noon	at	1 54	..... Ditto	24 20 24	
	Evening	at	— —	..... Ditto	— — —	Not obs.
Ditto in Oct.	Morning	at	8 45	..... Ditto	24 15 41	West.
	Noon	at	1 59	..... Ditto	24 22 53	
	Evening	at	— —	..... Ditto	— — —	Not obs.
Ditto in Sept.	Morning	at	8 53	..... Ditto	24 15 46	West.
	Noon	at	2 02	..... Ditto	24 22 32	
	Evening	at	6 03	..... Ditto	24 16 04	
Ditto in Aug.	Morning	at	8 44	..... Ditto	24 15 58	West.
	Noon	at	2 02	..... Ditto	24 23 32	
	Evening	at	7 05	..... Ditto	24 16 08	
Ditto in July.	Morning	at	8 37	..... Ditto	24 14 32	West.
	Noon	at	1 50	..... Ditto	24 23 04	
	Evening	at	7 08	..... Ditto	24 13 56	
Ditto in June.	Morning	at	8 30	..... Ditto	24 12 35	West.
	Noon	at	1 33	..... Ditto	24 22 17	
	Evening	at	7 04	..... Ditto	24 16 04	
Ditto in May.	Morning	at	8 22	..... Ditto	24 12 02	West.
	Noon	at	1 37	..... Ditto	24 20 54	
	Evening	at	6 14	..... Ditto	24 13 47	
Ditto in April.	Morning	at	8 31	..... Ditto	24 09 18	West.
	Noon	at	0 59	..... Ditto	24 21 12	
	Evening	at	5 46	..... Ditto	24 15 25	

Magnetical Observations continued.

Month.		Morning Observ.					Noon Observ.					Evening Observ.	
		Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Dec:	1	8h	55′	24°	18′	05″	1h	55′	24°	18′	55″		Discontinued, from the shortness of the days.
Ditto	2	8	45	24	18	00	1	50	24	20	20		
Ditto	3	8	50	24	23	42	2	15	24	29	35		
Ditto	4	8	55	24	19	17	—	—	—	—	—		
Ditto	5	8	50	24	20	56	1	55	24	22	41		
Ditto	6	8	50	24	17	40	1	55	24	19	20		
Ditto	7	8	55	24	16	39	2	05	24	20	10		
Ditto	9	8	55	24	16	56	1	55	24	21	36		
Ditto	10	8	50	24	17	43	1	55	24	20	20		
Ditto	11	8	50	24	18	02	1	50	24	20	44		
Ditto	12	8	05	24	16	17	1	45	24	19	14		
Ditto	13	8	00	24	18	15	2	00	24	18	34		
Ditto	14	8	55	24	18	38	1	55	24	19	50		
Ditto	15	8	50	24	15	40	1	45	24	22	31		
Ditto	16	—	—	—	—	—	1	50	24	18	15		
Ditto	17	8	50	24	15	50	1	55	24	16	57		

In taking the mean of the morning observations for the month of November, the extraordinary variation on the 11th is not included.

On December the 2d the needle vibrated between six and seven minutes; and during the night it blew very hard from the eastward.

Rain fallen { Between noon of the 1st Nov. / Between noon of the 1st Dec. } 0·695 inches.

Evaporation during the same period, 1·25 inches.

Article VIII.

Botanic Memoranda and Localities. Communicated by Mr. Winch, of Newcastle-upon-Tyne.

Salicornia Procumbens. Eng. Bot. t. 2475. Salt marshes at Saltholm, near Hartlepool, Durham; Mr. J. Backhouse, jun.: and on Tyne and Wear; N. I. W.

Salicornia Fruticosa. Shoreham Harbour, Sussex. Specimen from Mr. Borrer.

Hippuris Vulgaris. Ponds at Copgrove; Y.: and King's Park, Edinburgh; N. I. W.

Chara Nidifica. Eng. Bot. t. 1703. At Cocken-on-the-Wear. Weighill's Herbarium.

Chara Flexilis. Rivulet at the west end of Giggleswick Tarn, and in stagnant waters near Little Tarn; Y.: Mr. Windsor. Near

Yarmouth; Mr. Turner: mouth of the rivulet at Goswick, Northumberland; Thompson.

Zostera Marina. Growing on the shore at Inverary. Among the rejectamenta of the sea on the coast of Northumberland and Durham. N. I. W.

Ligustrum Vulgare. Hedges about Dorking, Surrey; N. I. W. St. Vincent's Rocks, Bristol; Mr. Thompson.

Circœa Alpina. Var. α between Keswick and Lodore. Var. β on the hill sides in Ashness Gill, Cumberland; N. I. W.

Veronica Spicata, naturalized. On the walls about St. John's College, Cambridge; N. I. W.

Veronica Hybrida. St. Vincent's Rocks, at the Giant's Hole, behind the Hot Wells; Mr. Thompson.

Veronica Saxatilis. Glen Tilt, north of Blair Athol; N. I. W. Observed in 1801.

Veronica Alpina. On Ben Lawer's; N. I. W.

Veronica Serpyllifolia, β; Fl. Brit. V. humifusa; Dickson: on Ben Lawer's Breadalbane, Cheviot, Northumberland; N. I. W.

Veronica Scutellata. At Hill Close Carr, Durham; Mr. Robson.

Veronica Montāna. High ridge wood near Settle; Y.; Mr. Windsor: Leith Wood and sea banks near Bristol; Mr. Thompson; woods in the neighbourhood of Newcastle, frequent. N. I. W.

Pinguicula Lusitanica. Nutshaling Common and Town Hill Common, Hants; Mr. Jos. Woods: Galloway, Scotland; Mr. M'Richie.

Pinguicula Vulgaris. Wallis, in the History of Northumberland, observes, "There is a variety in mountainous boggy meadows, with a very large flower of a duller purple, and a remarkable long spur." Can this be Pinguicula Grandiflora, Eng. Bot. t. 2184? N. I. W.

Utricularia Vulgaris. Pond in the Bull-close, Ripon; Y.; Mr. Brunton.

Utricularia Intermedia. Eng. Bot. t. 2489. In Prestwick Carr, Northumberland. This is the U. Minor of the Northumberland and Durham Guide, p. 3. N. I. W.

Utricularia Minor. Scotland; Mr. G. Donn: Burgh Common, Suffolk; Mr. J. Woods.

Lycopus Europæus. By the Mole at Brockham, Surrey; N. I. W. by the Avon at Bath; Mr. Thompson.

Salvia Verbenaca. On Brandon Hill, and below St. Vincent's Rocks, Bristol; Mr. Thompson.

Valeriana Rubra. Near Dartford, Kent, 1802; N. I. W.

Valeriana Pyrenaica. Eng. Bot. t. 1591. Woods near Edinburgh; Mr. Hooker.

Valeriana Locusta. In Heaton Dean; N.: about Dorking, Surrey; N. I. W.

Valeriana Dentata. Fields about Box Hill and Juniper Hill, Surrey; N. I. W.

Crocus Vernus. Harlestone, Norfolk; Mr. Turner.

Iris Fœtidissima. Near Ripon; Y.; Mr. Brunton.

Iris Germanica, blue flowered iris. In boggy places, but not common; Wallis's History of Northumberland. If Wallis observed any blue flowered iris in this county, it must have been iris fœtidissima; N. I. W.

Schœnus Mariscus. Conzyc Tarn. near Kendal, Westmoreland; Mr. Windsor.

Schœnus Nigricans. Highlands, frequent; N. I. W.: Burgh Common, Suffolk; and Town Hill Common, Hants; Mr. J. Woods: Bagshot Heath; Mr. Dickson: between Preston and Swinton; Y.; Mr. Windsor.

Schœnus Compressus. Ditches near Giggleswick Tarn, and banks of the rivulet opposite Gordale House; Y.; Mr. Windsor. Near Yarmouth; Mr. Turner: Bagshot Heath and Sydenham Common; Mr. Groult.

Schœnus Rufus. Thornton Lock, East Lothian, and Isle of Arran; Mr. M'Kay.

Schœnus Albus. About Southampton; M. J. Woods.

Schœnus Fuscus. Eng. Bot. t. 1575. Cromlyn Bog, near Swansea; Mr. E. Forster: Kerry, Galway, Ireland; Mr. J. T. M'Kay.

Scirpus Multicaulis. Chorley Common, near London; Mr. Groult: banks of the Derwentwater Lake, Cumberland; N. I. W.: Burgh Common, Suffolk; Mr. J. Woods.

Scirpus Pauciflorus. Nutshaling Town Hill, and Netley Commons, Hants; Mr. J. Woods, Dropmore Common, Bucks, ditto: Giggleswick Tarn; Y.; Mr. Windsor.

Scirpus Acicularis. Wanstead Park, Essex; Mr. E. Forster: Lochclunie, Scotland, N. I. W.: Giggleswick Tarn, at the mouth of the East Beck; Y.; Mr. Windsor.

Scirpus Fluitans. Near Yarmouth; Mr. Hooker: Staveley, Carrs; Y.; Mr. Brunton: Sales Moor, near Manchester; Mr. Robson: Pond at Forest Hall, Northumberland; N. I. W.

Scirpus Glaucus. Eng. Bot. t. 2321: Marshes at Dyke House, near Hartlepool; Durham: Mr. J. Backhouse.

Scirpus Carinatus. Eng. Bot. t. 1973: Battersea Fields, Surrey; Mr. Groult.

Scirpus Trequenter, α. Banks of Thames above London: Mr. Groult.

Scirpus Sylvaticus. Banks below Rangill in Bolland; Y.; Mr. Windsor. By the Mole at Brockham and Betchworth Park, Surrey; N. I. W. Ditches about Smockall Wood, near Bath; Mr. Thompson.

Eriophorum Polystachion. Bogs opposite Foal Foot, and North East side of Giggleswick Tarn; Mr. Windsor.

Eriophorum Alpinum. Restanant Moss, near Forfax; Mr. M'Kay.

Phalaris Phleoides. Cambridgeshire; Mr. Groult.

Alopecurus Bulbosus. Weymouth; Mr. Groult.

Alopecurus Fulvus. Eng. Bot. t. 1467. Swardenston, four miles from Norwich; Mr. Hooker.

Milium Lendigerum. Groom Bridge and other places about Hastings, Sussex; Mr. J. Woods.

Agrostis Panicea. Eng. Bot. Phleum Erinitum, Fl. Brit. Ditches at Halstow, Kent; Rev. J. Fenwick.

Agrostis Spica Venti. Fields at Old Windsor, Berks: on the Windmill Hills, Gateshead, Durham: by the road near North Shields and St. Anthons, Northumberland; N. I. W.: Copgrove; Y.; Rev. Mr. Dalton. Near London; Mr. Groult.

Agrostis Stolonifera. Eng. Bot. t. 1532. On the sea coast and in salt marshes. This grass puts out *strong sharp pointed shoots under ground*, as well as *runners* on the *surface*. When in seed its panicle is collapsed; N. I. W.

Agrostis Alba. Eng. Bot. t. 1189. By every ditch and road side, &c. &c. Throws out runners on the surface of the ground. Its panicle expanded when in seed. This is the Irish Fiorin I am certain, having cultivated plants which were brought from the fields of Dr. Richardson at Moy.

Agrostis Canina. This, in its autumnal state, is Agrostis Fascicularis, of Curtis; N. I. W.

Agrostis Vulgaris, γ; Fl. Brit. Agrostis Pumila; Lightfoot. An annual plant, and no variety of A. Vulgaris; N. I. W.

Agrostis Vulgaris, δ; Fl. Brit. Certainly A. Pallida with p. 128, t. 22; N. I. W.

Aira Cristata. Box Hill, Surrey; N. I. W.

Aira Levigata. Eng. Bot. t. 2102. Banks of Wear above Low Pallion, Durham, Sp. from Miss Pemberton.

(*To be continued.*)

Article IX.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, the 25th of November, the Bakerian lecture, on some electro-chemical phenomena, by Mr. Brande, was read. The identity of Voltaic and common electricity was first shown in 1801, by Dr. Wollaston. The object of the present lecture was to point out various other instances in which common electricity exhibits the same phenomena as Voltaic. It is well known that when bodies are exposed to the action of the Voltaic battery, oxygen and acids are attracted by the positive end; and alkalies, metals, and combustibles, by the negative end of the battery. Mr. Cuthbertson found that when the flame of a candle was placed between two excited balls, the negative ball was much more heated than the positive. This was explained by endeavouring to show that certain substances transmit only one kind of electricity. But it occurred to Mr. Brande that it was more probably owing to the carbonacious matter of the flame of a candle being, as in the Voltaic battery, attracted by the negative ball. Accordingly, a set of experiments were made, which confirmed this idea. The flame of phosphorus was most attracted to the positive ball, which of course became hottest. The same was the case with the flame of burning sulphur. The flame of hydrogen gas was attracted to the negative ball; that of sulphureted hydrogen, to the positive. Carbonic oxide flame was scarcely attracted to either. Olefiant gas was rather attracted to the negative, while the flame of phosphureted hydrogen was attracted to the positive ball. Benzoic acid sublimed was attracted to the positive ball, but its flame to the negative. Burning camphor was in like manner attracted to the negative ball. These and several other experiments confirm the opinion of Mr. Brande; though some phenomena occurred which he considered as anomalies not so easily applicable.

On Tuesday, the 30th of November, the Society met in order to choose the office-bearers for the ensuing year, when the following Gentlemen were chosen:—

The Right Hon. Sir Joseph Banks, Bart. President.

Dr. William Hyde Wollaston, and Taylor Combe, Esq. Secretaries.

Samuel Lysons, Esq. Treasurer.

Dr. Thomas Young, Secretary for Foreign Correspondence.

There were retained of the Old Council:—

Right Hon. Sir J. Banks, Bart.	John Pond, Esq. Astr. Royal.
Sir Charles Blagden, Knt.	Smithson Tennant, Esq.
The Lord Bishop of Carlisle.	Rev. Wm. Tooke.
Taylor Combe, Esq.	Wm. H. Wollaston, M. D.
Samuel Lysons, Esq.	Thomas Young, M.D.
Earl of Morton.	

There were elected into the Council:—

Mr. Astley Cooper.	Thomas Murdoch, Esq.
George Bellas Greenough, Esq.	Edward Rudge, Esq.
Thomas Harrison, Esq.	Sir Geo. Thom. Staunton, Bart.
Earl of Mansfield.	Wm. Charles Wells, M.D.
Francis Maseres, Esq.	Giffin Wilson, Esq.

The number of deaths since the last election was 13. The new members admitted during the course of the year amounted to 20. The Copleyan medal was given to Mr. Brande, for his papers on the existence of alcohol in fermented liquors, and for his other chemical papers published in the Philosophical Transactions. The ordinary members on the election list amount to 575, the foreign members to 43.

On Thursday, the 9th of December, part of a paper by Dr.

Brewster, containing further experiments on light, was read. The paper was divided into five sections. 1. *On the polarizing property of the agate.* When an agate is cut in a direction perpendicular to the direction of its plates, it has the property of polarizing light. Round the luminous object a nebulosity is seen which never disappears completely. If a prism of Iceland crystal be interposed and turned round, the nebulosity is found brightest when the luminous object disappears, and faintest when the luminous object is brightest. Hence Dr. Brewster concluded that the nebulosity was an imperfectly formed image of the luminous object, and that the agate was imperfectly crystallized, and possessed a certain degree of double refractive power. But upon attempting to separate the two luminous images by means of agate prisms, he could not succeed.

On Thursday, the 16th of December, Dr. Brewster's paper on light was continued. 2. *On the structure of the agate as connected with its action on light.* The fibrous and foliated structure of the agage, and the alternate zones of transparent and opaque portions, were particularly explained, and the effect which they produced on the rays of light shown. 3. *On the polarizing property of the agate.* Dr. Brewster had observed that on each side of the image seen by means of the agate there was a highly coloured image. This phenomenon is explained more in detail, and an attempt made to connect it with the structure of the agate. 4. *On the depolarizing property of bodies.* Dr. Brewster had formerly observed that when a ray of light is polarized, if it be transmitted through mica or topaz held in one particular direction, it is not altered; but if these bodies be placed in another direction, the light is depolarized. The first of these directions he calls the neutral axis; the second, the depolarizing axis of these bodies. He has since observed that almost all crystallized bodies possess this property of depolarizing light. He found it likewise in horn, gum arabic, and in some kinds of glass. He observed likewise that some crystals have the property of polarizing and depolarizing light on the same body. 5. *On the elliptical coloured rings exhibited by depolarized light.*

On Thursday, the 23d of December, the reading of Dr. Brewster's paper on light was concluded. He described various curious phenomena connected with the elliptical coloured rings; but we cannot pretend to make them intelligible without the assistance of figures. The paper concluded with a general recapitulation of the principal topics discussed in the essay, together with some other facts stated elsewhere. Among these may be mentioned the following: —The light from the clouds and the sky is mostly polarized. The rainbow consists of polarized light. Moon-light is not polarized.

At the same meeting a paper by Anthony Carlisle, Esq. was read, giving an account of a peculiarity of structure in the human body continued for four generations. This peculiarity exists in the family of Zerah Colburn, the American boy, exhibited in London about a year ago, and remarkable for his arithmetical powers. He came from the State of Vermont, in North America. He had six toes upon each foot, and five fingers upon each hand. An additional little finger growing out of the metacarpal bone of the little finger of each hand, and an additional little toe from the metatarsal bone of the little toe of each foot. His father has the same peculiarity. Zerah Colburn has five brothers and two sisters. The two sisters and two of the brothers have the natural number of the fingers and toes; three of the brothers are in the same predicament as Zerah. The peculiarity was brought into the family by Zerah's grandmother.

The Society, on account of the approaching holidays, adjourned for three Thursdays.

LINNÆAN SOCIETY.

On Tuesday, the 7th of December, the remainder of Mr. Keith's paper on the direction of the radicle and plumula of plants was read. He next considered the opinion of Mr. Knight, that the direction of the radicle was owing to gravitation. This opinion is inadequate to account for the phenomena, because it assigns no reason for the upright direction of the stem, which, according to the laws of gravitation, ought to proceed in the same direction as the root. Mr. Keith conceives that the root increases chiefly at the extremity, and that the additional matter is at first fluid. Hence would appear a reason why the root points downwards. The stem, on the other hand, increases by introsusception, which seems best calculated for an upward direction. But this explanation Mr. Keith thinks unsatisfactory, because the root of the misseltoe first ascends and then descends; and no theory can account for these opposite directions. Mr. Keith conceives that the direction of the plumula and radicle of plants must be resolved into *vegetable instinct*, precisely analogous, and equally inexplicable, with animal instinct.

On Tuesday, the 21st of December, part of a paper by M. Marschall Von Biberstein was read, on the genus *serratula*. It is well known that the genera serratula, carduus, and some neighbouring genera of Linnæus, are very imperfect; and that the species have by succeeding botanists been referred sometimes to one genus, and sometimes to another, according as they derived their character from one part of the flower or the other. The author conceives that the serratula of Linnæus ought to be divided into two genera, to one of which he applies the name serratula, while he distinguishes the other by a different name. He informs us that in part of his arrangement he had been anticipated by Decandolle, but not in the whole. He then gives a technical description of the different species of serratula.

GEOLOGICAL SOCIETY.

At a meeting on the 3d of December a paper entitled "Memoranda relative to the Porphyritic veins of St. Agnes in Cornwall" was read.

The veins described in this paper occur on the coast between St.

Agnes and Cligga Point, traversing or lying on the surface of rocks of tortuous killas, or clay-slate. The veins themselves vary in thickness from forty feet to half an inch. Their general character is porphyritic, consisting of a base composed of minutely aggregated quartz, mica, talcite, and probably felspar, in which are imbedded grains and crystals of quartz, felspar, chlorite, mica and talcite in small patches. Sometimes the porphyritic character is superseded by a more completely crystalline one approaching to granite, and containing small veins of tin stone. Sometimes again the veins consist of quartz and tourmalines, forming a rock very nearly resembling that of St. Roche.

The clay-slate adjacent to the veins is more crystalline than elsewhere, and sometimes is scarcely to be distinguished from gneiss. Mr. Conybeare considers the veins and the rock in which they occur to be of contemporaneous origin.

A paper was also read entitled "A Description of some Specimens from the neighbourhood of Cambridge, by Henry Warburton, Esq. M. G. S.

These specimens formed part of a bed of rubble covering the summit of a hillock of grey, or the lower chalk, about five miles S. W. of Cambridge. This hillock, like several others in the same county, is situated to the west of the great range of chalk, being surrounded by the blue marl or gault, as it is provincially termed, from which the overlying bed of chalk is separated by a thin bed of green sand. The rubble, besides consisting of chalk and flint, also contains shell lime-stone, angular pieces of green-stone, and certain organic remains belonging to older beds than the chalk; but as all these beds basset more or less to the west of the place where these fragments are now to be found, the circumstance is considered by Mr. Warburton as indicating an ancient current, the course of which was from west to east.

A paper was also read entitled "Observations on Glen Tilt," by Dr. M'Culloch, V. P. G. S.

That part of Glen Tilt which is the subject of the present paper, extends four or five miles from Forest Lodge to Gow's Bridge. It consists of primitive-slate, assuming the form of clay-slate, of mica-slate, and of hornblende-slate, with which are interstratified various beds of granular lime-stone, more or less micaceous. Near Gow's Bridge the stratification is perfectly regular and uninterrupted, but higher up towards the Lodge it is traversed by granite rock and an infinite multitude of granite veins of various sizes. Where this latter rock makes its appearance, the even course of the schistus is interrupted in proportion to the magnitude of the mass of granite. When the granite, slate, and lime-stone are in contact, the latter is highly indurated and penetrated by silicious matter.

ARTICLE X.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Mr. **SINGER** will commence his Lectures on the Experimental Sciences, to a limited number of subscribers, on Tuesday, the 18th of January.

Dr. Clarke and Mr. Clarke will begin their next Course of Lectures on Midwifery and the Diseases of Women and Children, on Monday, January the 24th, 1814. The Lectures are read at the house of Mr. Clarke, No. 10, Upper John-street, Golden-square, every morning, from a quarter past ten to a quarter past eleven, for the convenience of students attending the Hospitals.

Dr. Merriman, Physician-Accoucheur to the Middlesex Hospital, &c. will begin a new Course of Lectures, on the Theory and Practice of Midwifery, and the Diseases of Women and Children, at the above Hospital, on Monday, January the 24th, at half-past ten o'clock.

II. *Singular new discovered Body,*

About a fortnight ago Sir Joseph Banks received a letter from Sir Humphry Davy, who is at present in Paris, mentioning a newly discovered violet coloured gas, which had lately attracted the notice of men of science: but no particulars are given of its nature or production.

In the *Journal de Paris* for the 3d of December, it is stated, that a memoir on this substance, by Clement and Desormes, had been read before the French Institute, and the following circumstances respecting it are stated. It was discovered by M. Courtois, and was obtained from kelp. When heated to 158°, it is converted into a gaseous substance of a strong violet colour. It is not acted on by oxygen, charcoal, or a red heat. With hydrogen and with phosphorus it produces muriatic acid. It combines with the metals without effervescence. It combines also with the metallic oxides, and forms compounds soluble in water. With ammonia it forms a detonating compound.

This notice is rather enigmatical; but it would appear from it that the substance in question has many properties in common with chlorine. Hence it is probably a compound of chlorine and some other body. What is meant by saying, that with phosphorus it forms muriatic acid, I do not understand. A few weeks will probably put us in possession of the mode of preparing this substance, and of course enable us to examine it.

When ammoniacal gas comes in contact with the oxymuriate of sulphur, it assumes a violet colour of great intensity and beauty.

Whether this has any connection with the substance in question, time will determine.

III. *New Properties of Light.*

Dr. Brewster has favoured me with the following account of his new and curious experiments on the polarization of light:—

"I have found that *light transmitted obliquely through all bodies, whether crystallized or uncrystallized, suffers polarization like one of the pencils produced by double refraction*; and from a great number of experiments, I have been enabled to determine the law by which all the phenomena are regulated.

"If light is incident at any angle, except a right angle, upon the surface of a transparent body, a portion of the transmitted pencil will suffer polarization. The quantity of polarized light varies as the cotangent of the angle of incidence; and there is always a particular angle, depending on the refractive power of the body, at which the emergent light is wholly polarized. When the light is transmitted necessarily through several parallel plates, either in contact or at a distance, the cotangents of the angles of polarization are always to one another as the number of plates employed; and the number of plates multiplied by the tangent of the angle at which they polarize light is a *constant quantity*. If the angle of incidence exceeds the angle of polarization, the pencil will still emerge in a polarized state.

"A parcel of 8 plates of plate glass polarizes the transmitted light at an angle of 79° 11′, and at any angle of incidence greater than this.

"A parcel of 16 plates polarizes the light at any angle above 69° 4′, and

"A parcel of 47 plates at any angle above 41° 41′.

"Similar effects, varying however with the refractive power, are produced by plates of mica, by films of blown glass, by coats of grease, gold-beaters' skin, and even gold leaf itself.

"Malus's discovery of the polarization of light by reflection is, perhaps, one of the most brilliant discoveries that optics has ever received; but though it developed a new set of phenomena analogous to those produced by doubly refracting crystals, yet as the polarization was obviously effected by *reflection*, and not by *refraction*, it did not furnish any information respecting the methods by which these crystals polarized the transmitted light. The discovery, however, of the polarization of light by oblique refraction forms the connecting link between these two classes of phenomena, and holds out the prospect of a direct explanation of the leading phenomena of double refraction, of the polarizing power of the agate, and of the partial polarization of light by polished metals."*

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* From some things contained in the preceding statement, it is evident that Dr. Brewster is unacquainted with the fact that Malus nearly three years ago discovered

IV. *On the Method of drawing fine Platinum Wire.*

I have received the following letter on this subject, dated Edinburgh, Dec. 9, 1813:—

(To Dr. Thomson.)

SIR,

In trying Dr. Wollaston's method of drawing fine platinum wire, I had considerable difficulty in casting the silver about the wire. The following method is much easier than either boring the silver, or casting it round the wire:—Bind the silver into a tube, and draw it through a plate, as is done in making small hinges. A piece of platinum wire pushed into this kind of tube may be drawn to any degree of fineness.

In one instance it was drawn so fine that when put into the nitric acid, and the silver dissolved, the platinum remained in the form of black floculi in the liquid.

In attempting to hammer platinum in the gold-beaters' case, it was soon broken to pieces. It may be made as thin as gold leaf, by rolling it between a folded piece of sheet copper. When a number of platinum leaves are rolled at once, leaves of copper should be put between them, to prevent their adhering.* S.

V. *Astronomical Observations at Oxford.*

In consequence of the notice on the wrapper of the last Number of the *Annals of Philosophy*, respecting astronomical observations at Oxford, I have received two communications on the subject, equally remarkable for the difference of the information and of the style. One Gentleman informs me that the Observatory at Oxford is under the care of the Savilian Professor, that regular observations are made, and that they have it in contemplation to publish them, like those at Greenwich. The other Gentleman says, that my note respecting this subject would not have been written if I had had any value for my character; that if I had asked for information on the subject, I might have had some details that would not have

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that light is polarized by *refraction*, as well as reflection. As Malus's paper on this subject is short, and seems to be unknown in this country, I shall insert a translation of it in the next Number of the *Annals of Philosophy*. It will enable Dr. Brewster to judge how far he has been anticipated by the French philosopher.—T.

* I have reason to believe that my Correspondent's method is not so good as Dr. Wollaston's. Dr. Wollaston, I know, tried it without finding it to answer. The black flocks which appeared when the silver was dissolved in nitric acid were probably not owing to the fineness of the wire, but to the badness of the mode of drawing it. I think also there is strong reason to doubt the possibility of rolling out platinum to the thinness of gold leaf. All Dr. Wollaston's friends must have often seen the fine platinum leaves which he formed some years ago by the same process as that of my Correspondent. The platinum is obviously as thin as it will go, yet there is a prodigious difference between the thickness of these leaves and common gold leaf. Nor need we be surprised at this, for gold itself could not be rolled out nearly so thin as gold leaf. The method of making gold leaf is quite different.—T.

been uninteresting to my readers; but as I did not give myself the trouble of inquiring, but hastily concluded that a thing does not exist merely because I was not aware of its existence, he thinks it only necessary to subscribe himself Oxoniensis.

If this hasty Gentleman will give himself the trouble to peruse again the note in question, he will find that he had no reason whatever to be out of temper on the subject. The note was written in order to procure information; and the expected information *was procured*, if not from him, yet from another Gentleman, fully adequate to the task. A man's character would be in a sorry predicament indeed if he durst not venture to give an opinion, whether right or wrong, merely as an opinion, without any hazard of losing it.

VI. *Mineralogical Teachers.*

Mr. Giesecké has been elected Professor of Mineralogy to the Dublin Society.

A Mineralogical Lectureship has been lately endowed by the Crown, at Oxford, with a salary of 100*l.* a year. Mr. Buckland, of Corpus is appointed to the lectureship.

ARTICLE XI.

List of Patents.

William Summers, the younger, New Bond-street, ironmonger; for a method of raising hot water from a lower to an upper level, for baths, manufactories, and other useful purposes. Nov. 1, 1813.

Joseph C. Dyer, Gloucester-place, Camden Town; for a method of spinning hemp, flax, grasses, or any substance having considerable length of fibre: communicated to him by a foreigner residing abroad. Nov. 1, 1813.

Thomas Rogers, Dublin; for new flour for bread, pastry, and other purposes. Nov. 1, 1813.

Samuel James, Hoddesdon, Hertfordshire, surgeon; for a sofa for the ease of invalids and others. Nov. 1, 1813.

John Ruthven, Edinburgh, printer; for a machine, or press, for printing from types, blocks, or other surfaces. Nov. 1, 1813.

John Barton, Tufton-street, Westminster, engineer; for various improvements in the construction and application of steam-engines. Nov. 1, 1813.

Benjamin Sanders, Granby-place, Surrey, button manufacturer; for an improved method of manufacturing buttons. Nov. 4, 1813.

Charles Wilks, Ballincollig, county of Cork, Esq.; for a method of constructing four-wheeled carriages of all descriptions, whereby a facility of turning is obtained, without having recourse to the usual modes of having what is commonly called locks, or having any necessity for keeping the fore-wheels of such carriages lower than the hinder wheels usually are, or of raising the bodies of such carriages higher than usual. Nov. 9, 1813.

Richard Jones Tomlinson, Bristol, iron-master; for certain improvements in the methods of constructing or making the coverings of the roofs, or of other surfaces of buildings, whether external or internal. Nov. 13, 1813.

James Brunsall, Plymouth, tailor; for certain improvements in different stages of rope-making, and in machinery adapted for such improvements. Nov. 16, 1813.

William Pope, Bristol, perfumer; for an instrument or instruments, to be used jointly or separately, for ascertaining a ship's way at sea, and assisting in determining the longitude. Nov. 16, 1813.

William Barge, Bristol, confectioner; for certain improvements in the construction of fire-places. Nov. 16, 1813.

Edward Charles Howard, Westbourne-green, Middlesex, Esq.; for certain improvements in the process of preparing and refining sugars. Nov. 20, 1813.

Edward Biggs, Birmingham, brass-founder; for a method of working stamps by a steam-engine, water, or horse power. Nov. 23, 1813.

Frederick Cherry, Croydon, Surrey, veterinary surgeon to the army; for certain improvements in the construction of various articles of an officer's field equipage. Nov. 23, 1813.

James Bodmer, Stoke Newington, Middlesex; for a method of loading fire-arms, cannon, and all ordnance, except mortars, at the breech with a rifle or plain bore, and also a touch-hole for fire-arms and ordnance, and also a moveable sight for fire-arms and ordnance. Nov. 23, 1813.

Jeremiah Donovar, Craven-street, Strand, and **John Church**, Chelsea, soap-boiler; for saponaceous compounds for deterging in sea-water, hard water, and in soft water. Nov. 23, 1813.

Richard Mackenzie Bacon, Norwich, printer, and **Bryan Donkin**, Bermondsey, Surrey, engineer; for certain improvements in the implements or apparatus employed in printing, whether from types, from blocks, or from plates. Nov. 23, 1813.

John Duncombe, Woolwich, civil engineer; for an improvement to mathematical and astronomical instruments, in order to render them more portable, accurate, easy, expeditious, and certain in their application to topographical and nautical surveying, the mensuration of terrestrial and celestial angles, and the direct distances of inaccessible objects at one station by sea or land, without the usual modes of calculation; also the natural sine and cosine of such angles are precisely obtained to any eligible radius without tabular or other reference; also an improved compass, whose index points due north and south, and is capable of adjustment according to the known or observed variation of the needle. Nov. 25, 1813.

Article XII.

Scientific Books in hand, or in the Press.

Lord Glenbervie is preparing for publication a treatise, Practical and Experimental, on the Cultivation of Timber, particularly Oak, for domestic and naval purposes.

Mr. Salt's Second Voyage to Abyssinia is preparing for publication.

Mr. T. Baynton, of Bristol, will shortly publish a new and successful Method of treating Diseases of the Spine.

The Rev. John Toplis, B. D., Fellow of Queen's College, Cambridge, has in the Press a Translation of the Treatise upon Mechanics, which forms the Introduction to the Mechanique Celesté of P. S. Laplace. It will be accompanied by copious explanatory notes.

A New Edition of Key's Treatise on the Management of Bees, in a small volume, is nearly ready.

The second and concluding volume of Langsdorff's Voyages and Travels, containing his journey from Kamschatka to the Aleutian Islands, and the North West Coast of America, and return over land through Siberia to Petersburgh, will shortly be published. As will likewise,

The Travels of Julius Von Klaproth in the Caucasus and Georgia, undertaken by order of the Russian Government.

The Naturalist's Miscellany, published by the late Dr. Shaw, is shortly to be continued, under the title of the Zoologist's Micellany, by William Elford Leach, M. D. F. L. S. &c. and R. P. Nodder.

Mr. Sowerby, No. 2, Mead-place, Lambeth, has announced that, as soon as English Botany and British Mineralogy are finished, he will commence a work, to be written by Dr. Leach, of the British Museum, upon the Malacostraca Britannica, or British Crabs. He supposes the first Number will appear soon after March, before which time English Botany cannot be finished, on account of the difficulty of procuring the few Mosses yet unpublished. Mr. Sowerby also earnestly requests that mineralogists would send him, for the purpose of figuring, such newly discovered Minerals as may not already have appeared in the British Mineralogy. Localities of Fossil Shells for his Mineral Conchology would also be very acceptable.

*** *Early Communications for this Department of our Journal will be thankfully received.*

ARTICLE XIII.

METEOROLOGICAL TABLE.

		BAROMETER.			THERMOMETER.					
1813.	Wind.	Max.	Min.	Med.	Max.	Min.	Med.	Evap.	Rain.	
11th Mo.										
Nov. 15	N W	29·38	29·20	29·290	47	34	40·5	—		☾
16	N E	29.10	29·02	29·060	45	35	40·0	—	·13	
17	N W	29·46	29·02	29·240	43	30		—	—	
18	W	29·65	29·46	29·555	44	32	38·0	—		
19	S W	29·72	29·65	29·685	51	37	44·0	·10	·15	
20	S	29·86	29·72	29·790	56	49	52·5	—		
21	S	29·88	29·86	29·876	55	47	51·0	—		
22	N W	29·93	29·88	29·905	55	37	46·0	—		●
23	E	29·98	29·93	29·955	45	38	41·5	7		
24	N E	30·08	29·98	30·030	45	33	39·0	—	—	
25	N	30·08	30·05	30·065	43	37	40·0	—		
26	N E	30·05	30·02	30·035	45	31	38·0	—		
27	N E	30·02	29·88	29·950	43	28	35·5	—		
28	E	29·95	29·85	29·900	37	34	35·5	—		
29	E	29·90	29·85	29·875	40	25	32·5	—		
30	S E	29·85	29·50	29·675	35	29	32·0	—		
12th Mo.										
Dec. 1	E	29·50	29·42	29·460	37	33	35·0	—		☾
2	S E	29·42	29·09	29·255	40	37	38·5	·12	·12	
3	S E	29·37	29·09	29·230	43	40	41·5	—	—	
4	N E	29·44	29·37	29·405	44	40	42·0	—	—	
5	N	29·76	29·44	29·600	44	36	40·0	—	—	
6	N	29·78	29·77	29·775	44	36	40·0		·20	
7	N E	29·76	29·72	29·740	44	41	42·5	2	3	○
8	N E	29·86	29·72	29·790	44	40	42·0	—	—	
9	N E	30·05	29·86	29·955	44	39	41·5	—	—	
10	N E	30·18	30·05	30·115	43	37	40·0	4	·14	
11	N E	30·18	30·05	30·115	42	37	39·5	—		
12	S E	30·05	29·82	29·935	37	29	33·0	—		
13	N W	29·93	29·82	29·875	37	26	31·5	2		
		30·18	29·02	29·728	56	25	39·63	0·37	0·77	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Eleventh Month.—16. A stormy night, after some rain. 17. Much wind, a. m.: snow from 2 to 3 p. m. which remained through the night. 18. A fine day: at sun-set much rose colour, with *Cirrostratus*. 19. Overcast at 8 a. m., the barometer falling: misty and drizzling. 22. Grey morning: fair day. 23. Misty, calm: 24. Overcast, drizzling. 25, 26, 27. Fine days. At sun-set on the 27th the wind being S.E. the smoke of the city, in passing away in a body, swelled up into several distinct heaps, each of which inosculated at its summit with a small cloud. This union was clearly the result of mutual attraction, and it continued for great part of an hour. The clouds which at this time overspread the sky in great numbers were the remains of larger ones, which had in part evaporated, and now resembled *Cirrostrati*: those which were attached to the smoke became sensibly denser than the rest. 30. This month has exhibited an unusual proportion of fine days.

Twelfth Month.—2. Cloudy: a steady breeze: some light snow and sleet: rain in the night. 3. Overcast: some rain. 4, 5. Cloudy, misty: rain at intervals. 8. a. m. small rain.

RESULTS.

Prevailing Winds, Easterly.

Barometer:	Greatest height	30·18 inches;
	Least	29·02 inches;
	Mean of the period	29·728 inches.
Thermometer:	Greatest height	56°
	Least	25°
	Mean of the period	39·63°

Evaporation, 0·37 inch. Rain, 0·77 inch.

The latter part of this period has been remarkable for a general prevalence of the diseases commonly attributed to obstructed perspiration. The detail of these belongs properly to the medical reports: it may, however, be suitable here to point out the circumstances which appear to have contributed to this effect. I say *appear*, because there undoubtedly exist modifications of the air capable of exciting disease, to the discovery of which none of the present means of examination are competent.

The wind, during the time alluded to, came in a moderate stream from the eastward; the barometer, which had been depressed, rising gradually. The sky was almost constantly overcast with *Cirrostratus*; beneath which the atmosphere was perceptibly full of diffused water, of the density of *dew*, quite down to the earth. The sun's rays thus intercepted, the temperature varied little from 40° by day or night; and evaporation was nearly at a stand. Electricity in such circumstances could not accumulate: hence, though it drizzled at intervals, the air never got cleared by showers.

Let us now apply these facts to the case. In health, the matter of perspiration is thrown out on the skin in a fluid state, in quantity and with a force proportioned to the state of the circulation in these respects. Here it has to evaporate in the common way of fluids; but it will do this very slowly, even at the common temperature of the skin, in an air already loaded with moisture. In such an air, too, the whole muscular system being relaxed, the heart and arteries act with less force. If it be, at the same time, only moderately cold, and void, in great measure, of light and electricity, it will want the exciting action of the nerves, which results from the *sudden* loss of heat, as well as from the application of the two latter *stimuli*. In a word, such an air, succeeding to a dry and clear air, will be a *sedative*: it will counteract, by constant, though insensible, effects, the healthy energies even of the strong. The usual *sponging* operation on the skin being withheld, at the same time that the *vis a tergo* is lessened, we need not wonder that the matter of perspiration should stagnate in the fine extremities of the cuticular arteries, or be thrown on some internal secreting surface; that the skin should take on a state of spasm, and that fever and local inflammation should ensue. Thus, without supposing in this case any occult quality in the air, we may trace a combination of qualities which may be reasonably thought to have made it productive of disease.

TOTTENHAM, *Twelfth Month*, 23, 1813. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1814.

ARTICLE I.

Memoir on New Optical Phenomena. By M. Malus. Read before the French Institute on the 11th March, 1811.*

I ANNOUNCED, at the end of 1808, that light reflected by all opaque or diaphanous bodies acquired new and very extraordinary properties, which distinguished it essentially from light directly transmitted from luminous bodies.

The observations which I now have the honour to report to the Class, are the *suite* of those which have been already communicated. I shall begin therefore with recapitulating, in a few words, the principal phenomena, in order to throw more light on the new experiments, and new results, which I am about to describe. Let us, by means of a heliostate, throw a ray of solar light in the plane of the meridian, so that it makes with the horizon an angle of 19° 10′. Let us, then, fix a mirror glass not silvered, so that it shall reflect this ray vertically downwards. If we place below this first glass and parallel to it a second glass, this second glass will make with the descending ray an angle of 35° 25′, and will reflect it anew, parallel to its first direction. In this case we shall not observe any thing remarkable; but if we turn round this second glass in such a manner that its face shall be directed towards the west or towards the east, without changing its inclination with respect to the vertical ray, it will no longer reflect a single particle of light either at its first or second surface. If (still preserving the same inclination with respect to the vertical ray) we turn its face to the south, it will begin anew to reflect the usual proportion of the incident light. In the intermediate positions the reflection will be more or less complete, according as the reflected ray approaches more or less to the plane of the meridian. In these circumstances, though the reflected ray acts so very different a part, it preserves always the same inclination with respect to the incident ray. We see here, therefore, a vertical ray of light, which falling upon a diaphanous body acts in one way when the reflecting face is turned to the north or south, and in a different way when that face is turned towards the east or west, though these faces form always the angle 35° 25′ with the vertical direction of that ray. These observations lead us to conclude that the light acquires, in these circumstances, properties independent of its direction with respect to the reflecting surface, and connected solely with the sides of the vertical ray, which are the same for the north and south sides, and different for the east and west sides. Giving to these sides the name of *poles*, I shall call the modification which gives to light properties relative to these poles *polarization*. I have hitherto hesitated to admit this term into the descriptions of the physical phenomena here described. I did not venture to introduce it into the Memoirs in which I published my first experiments; but the varieties which this new kind of phenomenon present, and the difficulty of describing them, oblige me to admit this new expression, which signifies simply the modification which light has undergone in acquiring the new properties which are not relative to the direction of the ray, but only to its sides considered at right angles, and in a plane perpendicular to its direction.

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* Translated from the Moniteur of the 13th March, 1811.

I now pass to the description of the phenomenon which constitutes the object of this memoir. Let us consider anew the apparatus of which I have just spoken. If we present to the solar ray which has traversed the first glass, and of which a part has been reflected, a silvered mirror which will reflect it downwards towards the ground, we obtain a second vertical ray, which has properties analogous to those of the first, but in a direction exactly opposite. If we present to this ray a glass forming with its direction an angle of 35° 25′, and if without changing this inclination we turn the faces alternately to the north, east, south, and west, we shall observe the following phenomena. There will always be a certain quantity of light reflected by the second glass; but this quantity will be much less when the faces are turned to the south and north than when they are turned to the east and west. In the first vertical ray we observed exactly the contrary. The minimum of reflected light was when the second glass was turned to the east aud west. Therefore if we abstract in the second ray that portion which acts as ordinary light, and which is equally reflected in all directions of the face, we see that this ray contains another portion of light which is polarized exactly in the opposite way of the vertical ray reflected by the first glass. In this experiment I employ a silvered mirror merely to dispose the two rays parallelly, and in the same circumstances, in order to render the explanation more clear. The action of metallic surfaces being very feeble relative to the polarization of the direct ray, we may neglect their influence.

This phenomenon may be ultimately reduced to this: when a ray of light falls upon a glass forming with it an angle of 35° 25′, all the light which it reflects is polarized in one sense. The light which passes through the glass is composed, 1. Of a quantity of light polarized in the opposite sense to that which has been reflected, and proportional to that quantity. 2. Of another portion not modified, and which preserves the characters of direct light. These polarized rays have exactly all the properties of those which have been modified by doubly refracting crystals. Hence what I have said elsewhere of the one may be applied to the other.

The following are the general results which may be drawn from the experiments of which I have given an account, and which ought to be added to those which I have already published on this subject.

Whenever we produce, by any means whatever, a polarized ray, we necessarily obtain a second ray polarized in a direction diametrically opposite; and these rays follow different routes. Light cannot receive this modification in one direction without a proportional part of it receiving it in an opposite direction.

The curious observation which M. Arrago has lately stated to the Class would seem alone, at first view, to constitute an exception to this general rule. He has remarked, that the coloured rings, by transmission, presented the phenomena of polarization; and in this case the most distinct zones appear polarized in the same direction as the reflected light: but if we reflect upon the cause of this phenomenon, we shall perceive that it is not an exception to the general rule.

All bodies, both opaque and diaphanous, polarize light under all angles, though for each of them this phenomenon is at a maximum at a particular angle. We may therefore say, in general, that all light, which has experienced the action of a body by reflection or refraction, contains polarized rays, whose poles are determinate relative to the plane of reflection or refraction. This light has properties and characters which the light has not that comes to us directly from luminous bodies.

I subjected to the same trials the coloured zones formed by the dispersion of light when it passes very near opaque bodies: but I have not hitherto made a single remark worth reporting to the Class.

I shall add to these observations the result of some researches which I formerly announced on the same subject. I have determined with respect to many substances the angle of reflection at which the incident light is most completely polarized; and I have ascertained that this angle neither follows the order of the refractive powers, nor that of the dispersive forces. It is a property of bodies, independent of the other modes of action, which they exercise on light. After having determined the angle under which this phenomenon takes place with respect to different bodies, water and glass, for example, I sought for that at which the same phenomenon would take place at their surface of separation, when they are in contact: but the law according to which this last angle depends upon the first two still remains to be determined.

I published, a year ago, in the Memoirs of the Society d'Arcueil, that after having modified a solar ray I made it pass through any number of diaphanous bodies without any of it being reflected, which gave me a method of measuring exactly the quantity of light that these bodies absorb—a problem which the partial reflection of light by bodies rendered of impossible solution.

Thus, by placing in the direction of a polarized ray a pile of parallel glasses forming with it an angle of 35° 25′, I had observed that this ray produced no reflected light upon any of them; and I concluded from this that the light, which would have been reflected on employing a common ray, in this case passed through the diaphanous bodies. A foreign philosopher, in giving an account of my experiment, observes, that he is not of my opinion that the modified light is transmitted by the surfaces when it is not reflected, and that he is rather inclined to believe that in this case the portion reflected in ordinary cases is entirely absorbed or destroyed. I have solved that question decisively by the following experiment: I made the incident ray turn round without changing place, and preserving the same position with respect to the pile. When it has made $\frac{1}{4}$th of the circumference, it is totally reflected by the successive action of the glasses, and it ceases to be seen at the extremity of the pile. After half a revolution on itself it begins to be transmitted anew. This experiment presents the singular phenomenon of a body which appears sometimes diaphanous and sometimes opaque, while it receives not only the same quantity of light but the same ray, and under the same inclination.

It is needless to observe, that in order to make a polarized ray turn round, I employed a ray formed by the ordinary refraction of Iceland crystal, whose faces are parallel to each other, and perpendicular to the direction of the ray. By turning these faces in their own plane, I change the position of the poles of the ray without varying its direction or intensity.

I shall not detail the consequences that may be deduced from what I have stated: all that I could add would be a repetition of the same facts presented in a different manner.

ARTICLE II.

Remarks on the Hypotheses of Galvanism. By J. Bostock, M.D. M.G.S. &c. &c.

(*Continued from* p. 42.)

THE electrical hypothesis being not adequate to account for the phenomena of the Galvanic pile, we must substitute for it the chemical hypothesis, which supposes that the first step of the process consists in the action of the fluid upon one of the metals, and that the electrical phenomena depend upon this action. This action essentially consists in the oxidation of the surface of one of the metals, while the opposite surface of the other metal is not oxidated. The positions upon which the chemical hypothesis rests are the following:—1. That a metal (the zinc, for example,) is oxidated, the oxidated part has its capacity for electricity diminished, and electricity is consequently evolved. 2. This electricity is received by the contiguous fluid, and is transmitted by it to the other metallic surface, the copper, which is not oxidated, and is therefore disposed to receive it; and the whole of the copper plate hence becomes positive. 3. The remaining part of the zinc, which is not oxidated, remains in its natural state; and therefore, as it relates to the copper, is negative. 4. The elements of the pile, which, according to the electrical hypothesis, are supposed to be in the order of copper, zinc, fluid; are, according to the chemical hypothesis, zinc, fluid, copper; the electricity passing from the first zinc plate, across the fluid, to the copper plate. 5. The action takes place, not between the metals, but between the oxidated surface and the fluid; no change would therefore be produced by placing a copper plate beyond the first zinc plate, or a zinc plate beyond the last copper plate. Strictly speaking, it is the zinc end of the apparatus which is negative; and the copper, positive.*

The chemical hypothesis accounts for all the effects of the Galvanic pile, and appears to be consistent with itself in all its parts. It explains satisfactorily why the action of the pile is always in proportion to the oxidation of the metals, a fact pointed out and insisted upon by Dr. Wollaston.† The progressive increase in the action of the different parts of the apparatus is easily explained. The first copper plate, in consequence of the electricity which it has acquired from the oxidated part of the zinc, becomes positive; or, to use the numerical illustration, is brought to the state of 110°. This state it communicates to the contiguous, or second zinc plate, which also becomes 110°. The fluid oxidates the surface of the metal here, as in the former case; but a larger quantity of electri-

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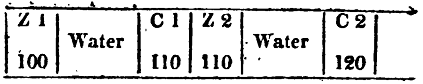
* See Erman, Lehot, De Luc, &c.

† Phil. Trans. 1801.

Fig. 4.



city is liberated, which is transmitted to the second copper plate, and which raises it to 120°. (*Fig.* 4.) According to the chemical hypothesis, although one end of the pile becomes highly positive, and is therefore disposed to communicate a portion of its redundant electricity to the other end, yet this end may be considered as only relatively negative, and there is no part of the apparatus in a neutral state. There is no tendency in any part of the apparatus to restore the equilibrium of the electric fluid, which is destroyed by the oxidation of the metal. The sole effect is to increase the electricity of one end of the apparatus; and therefore the more powerful is the action of the fluid, either in oxidating the metals, or in conveying the electricity to the opposite surface, the more powerful will be the effect of the apparatus. This effect does not necessarily depend upon the ends of the pile being united, because the essence of the operation consists in the oxidation of one of the metals. It is, however, to be expected, that the effect will be increased when the extremities of the pile are united; for in this case the electricity will be powerfully attracted towards one end of the apparatus, in order to produce an equilibrium with the other end, which is relatively negative. The chemical hypothesis is not encumbered with the necessity of a current of electricity from one end of the pile to the other; it only supposes the passage of the electricity across the interposed fluid in each individual pair of metals. The chemical differs from the electrical hypothesis in the material circumstance of the former pointing out a source for the liberation or evolution of electricity, in consequence of a change in the nature of one of the substances, which renders it less disposed than before to contain it. The electrical hypothesis only contemplates an interchange of electricity between the different parts of the apparatus, one part losing a portion merely in consequence of the other part attracting it, a process by which there can be no *absolute production* of electricity. The chemical differs from the electrical hypothesis with respect to the state of the contiguous metals: the electrical hypothesis supposes that they can have different states of electricity while in contact; the chemical takes it for granted that while they are in contact their electrical states must be similar. The chemical hypothesis explains all the facts that have been observed respecting the necessity of oxygen for the action of the apparatus, it explains the reason why the metals must differ in their degree of oxidability, and why the fluid must be one which will act differently upon the two metals. The facts which have been noticed respecting the different effects of the interposed fluids may be explained by referring to three circumstances, which all coincide with the chemical hypothesis. 1. That the fluid acts only upon one of the metals. 2. That the surface of one of the metals only is oxidated with a certain degree of rapidity. 3. That the oxide is removed so as to expose a fresh surface to the fluid. If acids be employed, those are the best that dissolve the oxide; or if neutral salts, those which form triple salts with the oxide which is produced. The chemical hypothesis affords a plausible method of accounting for the different effects of the apparatus, whether we use large or small plates; for it is not unreasonable to suppose that the electricity will become more intense or concentrated at every successive transmission through a new oxidating surface, while its absolute quantity depends upon the amount of oxide that is formed.

With respect to the experiments of Bennett, or others of a similar kind, which have been adduced in favour of the electrical hypothesis, it is probable that Volta has been mistaken in his application of them, and of the principle to which they should be referred. Instead of supposing, as he does, that all metals are naturally in the same state of electricity, and that, by being placed in contact, a portion of it is attracted from one to the other, the facts would seem to indicate that the reverse takes place; and that although the electricity of each individual metal is in equilibrio with the atmosphere, yet that it is unequal with respect to each other; that zinc, for example, is negative with respect to copper. When they are placed in contact, their electricity is equalized; but when they are again separated, the zinc, having acquired a portion of electricity from the copper, becomes positive with relation to the atmosphere; and the copper, for the same reason, negative.

The effect that takes place in the interrupted circuit, between the extremities of the pile, consists in the decomposition of the interposed substance, and in the discharge of its constituents from the two wires. If we take water as an example, and wires are employed which are not acted upon by the substances liberated, we find that the fluid diminishes in bulk, while from one end oxygen is discharged, and from the other hydrogen, and these in the proportion requisite to form water. I made an attempt to analyse the nature of this operation, by supposing that when electricity is discharged from the wire connected with the positive end of the pile, it unites itself to a portion of hydrogen, in order to pass across the fluid to the negative extremity. For this purpose it decomposes a quantity of the water contiguous to the wire, and liberates the oxygen; and after passing across the fluid in connection with the hydrogen, it discharges the hydrogen upon entering the negative wire.* A different principle was advanced by Hesinger and Berzelius, which was afterwards adopted and extended by Sir H. Davy. It supposes that when electricity is transmitted through a fluid, the fluid becomes affected in such a manner that the elements of which it is composed are attracted to the different wires, and the fluid itself decomposed; the hydrogen, and all inflammable substances, passing to the negative; while oxygen and acids are attracted to the positive wire.*

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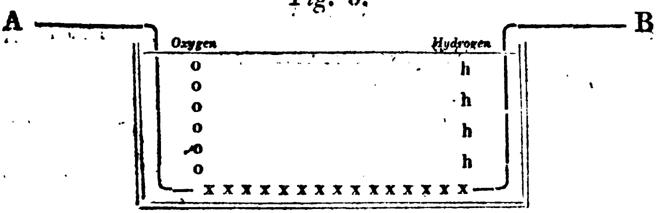
* Nich. Jour. iii. 9. Long after the publication of my former remarks on Galvanism, I met with a passage in the Journal de Physique, which proves that a very similar idea had occurred to MM. Fourcroy, Vauquelin, and Thenard. M. Delametherie, who writes the article, says, that these philosophers supposed that the electricity passes from the positive to the negative wire, decomposes the water, leaves the oxygen, and carries the hydrogen to the negative wire, where it leaves it. (a) It is not stated whence this opinion is derived, nor have I been able to ascertain this point. See also some observations of Mr. Cuthbertson's. (b)

(a) Jour. de Phys. liv. 15.

(b) Nich. Jour. ii. 287.

I shall now attempt to explain the nature of the effect which must be produced upon the water according to the first of these hypotheses. The positive wire, A, and the negative wire, B, being immersed in water, the electricity rushes from A to B to produce an equilibrium between them, and this current of electricity is kept up in consequence of the continued evolution from the body of the pile. But electricity cannot be conveyed through water except in combination with hydrogen: it therefore decomposes the particles of water that are contiguous to the point of the wire, takes possession of the hydrogen, and discharges the oxygen; it then crosses the fluid, and discharges the hydrogen at the negative wire. There will be a continued current of hydrogen at all times passing through the fluid; but being in combination with electricity, it does not display its usual affinities. This current of hydrogen must exist in an equal degree in all parts of the water interposed between the wires, and must continue as long as the pile continues to act. It is obvious that there can be no current of oxygen through any part of the water, except from the end of the positive wire to the surface; nor can there be any accumulation of either this substance or hydrogen, except perhaps at the immediate termination of the wires. The discharge of the oxygen and hydrogen from the two wires respectively is supposed to depend, not upon any direct action which the wires exert on the water, but upon the electricity abstracting a portion of hydrogen which it afterwards liberates. (*Fig.* 5.)

Fig. 5.



The mark x is intended to express the union of hydrogen and electricity.

According to the other hypothesis, the effect produced depends upon the attraction which the two wires possess for the two constituents of the water, the positive wire for the oxygen, and the negative wire for the hydrogen; so that as soon as A and B are immersed in the water, in consequence of their attraction for its constituents, they each begin to act upon the parts contiguous to them, A attracting the oxygen, and B the hydrogen. But no sooner have the wires decomposed the water, in consequence of their attraction for its constituents, than the substances attracted are disengaged, while the other constituent remains in the water; so that the fluid on the side near the positive wire will contain an excess of hydrogen; and that near the negative wire, of oxygen. These liberated particles of hydrogen and oxygen are supposed to be repelled from the wires near which they are situated, to meet in the centre of the fluid, combine again, and reproduce water. This, I believe, will be admitted to exhibit a fair view of the hypothesis as detailed by Sir H. Davy; and yet, I confess, it has always appeared to me to involve many improbabilities, if not contradictions. The fundamental principle upon which it rests is, that the positive wire attracts oxygen; and the negative, hydrogen; and, in consequence of these attractions, that the water is decomposed; yet the effect produced upon the water is, that the oxygen is discharged by the positive wire and the hydrogen retained, and the hydrogen discharged from the negative wire and the oxygen retained. But a repulsive force is supposed to be exerted here as well as an attractive one, and the hydrogen is repelled from the positive wire at the same time that the oxygen is attracted by it; yet the same peculiarity occurs here as before, the substance that is repelled by the wire is retained in the water, while that which is attracted by it is discharged. It follows, according to this hypothesis, that the different parts of the water must be in different states of chemical combination, the part near the positive wire containing an excess of hydrogen, and that near the negative wire an excess of oxygen: and the hydrogen and oxygen are here in an uncombined state, and therefore they might be expected each of them to exhibit their specific chemical properties, or at least to affect the chemical composition of the water with which they are united. This hypothesis necessarily supposes the existence of two currents through the fluid, one of hydrogen from the positive, and the other of oxygen from the negative wire, which meet in the centre, and are there mutually destroyed. These opposite currents depend partly on the repulsion exercised by the wires on the constituents of the water, and partly on their attraction for each other. Hence it may be inferred that if some substance were presented to them in their course, to which they had a stronger attraction than that which they possess for each other, the water would not be reproduced; and if one of them only was attracted in this way, the other would be discharged. This will appear more plain if we suppose that the two wires terminate in two separate quantities of water, that are connected by a tube or any fibrous substance, so as to form a communication between the two portions. The recomposition of the water will take place only in this tube, which is supposed to be exactly in the centre, and the whole of the fluid in contact with the positive wire will be hydrogenated, and the whole of that in the negative wire oxygenated. If, then, we were to place any substance in the hydrogenated water which had an attraction for hydrogen, a union should take place, an effect with which the repelling force of the positive wire would co-operate. In this case the particle of oxygen repelled from the other wire, not meeting with any hydrogen, must in some way or other be discharged from the oxygenated water. It does not, however, appear that any facts of this kind have ever been observed; on the contrary, the water does not seem to possess any difference of chemical composition in its different parts, except immediately about the extremities of the wires. (*Fig.* 6.)

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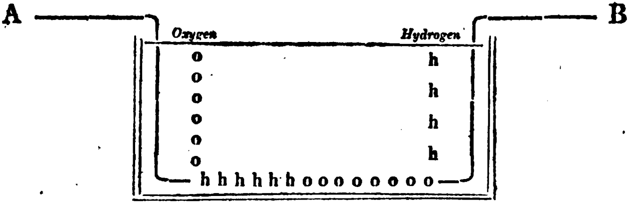
* Ann. de Chim. li. 167.

Fig. 6.



This view of the nature of the two hypotheses leads me to the conclusion, that the former of them is more simple and consistent in its different parts, and that it is free from some difficulties which attach to the latter.

The evidence for the truth of the electrical hypothesis is in some degree connected with the views which have been lately taken by Sir H. Davy respecting the connection between electricity and chemical affinity; it may be proper, therefore, to consider the grounds upon which these opinions rest, and also to inquire how far they affect the present question. Hesinger and Berzelius seem first to have distinctly pointed out the effect which the two extremities of the pile possess of attracting to themselves different kinds of substances; and Sir H. Davy afterwards, in a series of very elaborate experiments, showed, that this action was so powerful as apparently to counteract the usual effects of chemical affinity. Not only was a solution of a neutral salt, in which the two wires terminated, decomposed, and the acid attracted to the positive, and the alkali or metallic ingredient to the negative wire; but the wires seemed even to possess the power of attaching to themselves the acid and the alkali, when other substances intervened, to which they each had a strong affinity. These effects were attributed to the attraction of the positive wire for substances containing oxygen, and of the negative wire for those that contained hydrogen or any other inflammable ingredient; and Sir H. Davy was induced to infer, that the former class of bodies were naturally in a negative state of electricity, and the latter in a positive state. This conclusion, with respect to the natural electrical condition of these two

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classes of bodies he afterwards attempted to substantiate by more direct experiments, in which the electrometer indicated the negative electricity after being in contact with acids, and the positive after being in contact with alkalies.

Two distinct sets of facts and experiments are here brought into view, those regarding the decomposition and transfer of acids and alkalies by the two extremities of the pile, and those to which the electrometer was applied: it is to the former of these only that the present question has any reference. Before we can form a correct judgment concerning them, it will be necessary to attend a little more minutely than has hitherto been done to the electrical state of the water in the interrupted circuit. When water, or any other conductor, is interposed between two substances that are in different states of electricity, its first effect is to form a communication between them, by means of which their electricities are equalized, the water itself, as well as the two electrified bodies, all acquiring the same degree of electricity. When the two extremities of the pile are connected by the intervention of water, there is, in the first instance, an attempt to produce this equilibrium; but the equilibrium is no sooner formed than it is again destroyed by the continual generation or evolution of electricity which goes on in the body of the pile. On this account the wire which was originally positive is kept in that state, and the same with respect to the negative wire. But the two wires being immersed in water must have a constant tendency to bring the water into the same electrical state with themselves, and must, to a certain degree, accomplish it; so that we may conclude, that the water contiguous to the positive wire is itself positive, and that contiguous to the negative wire negative. These two portions of water must, however, be continually tending to equalize their electrical states with that of the remaining part of the water, and the result will be, that contiguous to the two wires there are two portions of water, in the same, or nearly the same, electrical states with the wires themselves, and that the electricity diminishes in the successive portions of water, until at length, in the middle of the vessel, the fluid is in a neutral state. Now if we dissolve a quantity of a neutral salt in this water, we find that the acid particles diffuse themselves through the water which is positively electrified, and the alkaline particles through that which is negatively electrified. This is well illustrated in the experiments of Sir H. Davy, where the water was tinged with litmus and turmeric; for it was found that the intensity of the effect produced on the colours, and consequently the quantity of acid and alkali contained in the water, was greatest near the wires, and diminished until it arrived at the centre, where the effect ceased. The transfer of the acid to the neighbourhood of the positive wire may, therefore, with greater probability, be ascribed to its being attracted by the positively electrified water, than to the positive wire itself; for it does not attach itself immediately to the wire, but it diffuses itself through the water, in proportion to the electrified state of its different parts. If we go farther, and inquire why acids are disposed to be transferred to water which is in a state of positive electricity, we may, I think, fairly conjecture, according to Sir H. Davy's hypothesis, that it is owing to the attraction which exists between oxygenated substances and positive electricity, as the acquisition of this electricity seems to be the only change which the water has experienced.

From this view of the subject I am led to conclude, that when the extremities of the pile are connected by water, there are two distinct operations going forwards in the fluid; the first is the extrication of a quantity of oxygen from the positive wire, in consequence of the hydrogen which the electricity requires to enable it to pass through the water, and the subsequent extrication of this hydrogen at the negative wire; the other is the communication to the water of the electrical state of the wire which is in contact with it; it is to the first of these circumstances alone that the decomposition of the water and the disengagement of the gaseous products are owing, and to the latter alone, the decomposition of neutral compounds, and the transfer of their constituent parts to the different portions of the water. It will be admitted, that there is nothing in the nature of these facts which is at all contradictory to the chemical hypothesis; they are, indeed, so far favourable to it, as that they lead us to conclude, that the same kind of effect is carried on between every pair of plates as between the two terminating wires, except that the electrical fluid having a larger space to pass through, its operations are kept more distinct from each other.*

A large part of the reasoning in this paper is confessedly hypothetical; but if the hypothesis be a fair deduction from acknowledged phenomena, it will be entitled to some consideration. Scientific investigations are seldom performed to much advantage, unless there is some object in view farther than the mere observance of casual phenomena, while a correct hypothesis leads directly to the discovery of truth, by pointing out the track which we must pursue in our future experiments.

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* In treating upon the hypothesis of the Galvanic pile it would be impossible to omit noticing the elaborate essays which have been written upon the subject by M. De Luc. (*a*) His experiments are many of them very curious and valuable, and to some of his deductions I fully assent. Yet I am not disposed to coincide with him in his general hypothesis, particularly in that part of it where he endeavours to show that the chemical and electrical effects of the pile depend upon actions essentially dissimilar from each other; I think they may be all referred to a difference either in the quantity, intensity, or velocity of the electric fluid. The experiments prove that the course of the electricity through water is from the positive to the negative wire, the former disengaging oxygen, and the latter hydrogen, but no attempt is made to give any explanation of this process.

(*a*) Nich. Jour. xxvi. and following volumes.

ARTICLE III.

Essay on the Cause of Chemical Proportions, and on some Circumstances relating to them: together with a short and easy Method of expressing them. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

(*Continued from* p. 62.)

C.—*The Metals.*

1. *Arsenicum, arsenic* (As).—The experiments of Thenard, Proust, and Bucholz, as well as those which I have published, on the capacity of saturation of arsenic acid, seem to prove that arsenic combines with $\frac{1}{3}$ of its weight of oxygen to become the acid in *ous*, and with $\frac{1}{2}$ its weight to become acid in *ic*. In my experiments on the arseniate of lead I found that arsenic acid ought to contain twice as much oxygen as the oxide by which it is neutralized: but as, on the other hand, the relation between the quantities of oxygen found in the two acids shows that arsenic acid must contain at least 3 volumes of oxygen, it appeared to me that the neutral arseniates offered an exception to the general relation between the volumes of oxygen in the acid and those in the base, an exception which deserved to be examined with care. I determined, then, to repeat my experiments on the arseniates, in such a manner as to be certain that I operated upon perfectly neutral arseniates, and free from all mixture of arsenious acid.

On examining a portion of the arseniate of soda which I had employed in my former experiments for preparing arseniate of lead, I found, to my great surprise, that this salt, which had been carefully crystallized, gave unequivocal signs of containing an excess of alkali. It had effloresced a little; and when I neutralized it with arsenic acid, I found it no longer capable of crystallizing.

I prepared a portion of arseniate of soda, and to be quite certain that it contained no arsenious acid I fused it with nitrate of soda. The fused mass was dissolved in water, and rendered neutral by the addition of a few drops of nitric acid. With this solution I precipitated neutral solutions of nitrate of lead and muriate of barytes.

Arseniate of Lead.—I dissolved 10 parts of arseniate of lead in nitric acid, and then precipitated the solution by means of sulphate of ammonia. The sulphate of lead thus obtained being washed, dried, and heated to redness, weighed 8·865 parts. The acid solution from which this precipitate fell being neutralized by ammonia yielded a precipitate, which was arseniate of lead. Treated anew with sulphuric acid I obtained 0·088 part of sulphate of lead; so that 100 parts of arseniate of lead yielded 89·53 parts of sulphate of lead, equivalent to 66 parts of oxide of lead. Hence arseniate of lead is composed of

Arsenic acid	34	100
Oxide of lead	66	194·11
	100	

Now these 194·11 parts of oxide of lead contain 13·878 parts of oxygen. These, whether multiplied by two or three, give no number which corresponds with the composition of arsenic acid as above stated.

Arseniate of Barytes.—When the solution of arseniate of soda was mixed with that of muriate of barytes, no precipitate fell at first; but the arseniate of barytes gradually deposited itself in the state of small crystalline scales, which appeared quite insoluble in water: 10 parts of this arseniate heated to redness, and afterwards dissolved in nitric acid, produced with sulphuric acid poured into the solution 8·693 parts of sulphate of barytes. On repeating the experiment, I obtained 8·65 parts of sulphate of barytes. Supposing this last salt to contain 65·6 per cent. of barytes, arseniate of barytes is composed of

Arsenic acid	42·974	100·00
Barytes	57·026	132·70
	100·000	

According to the second experiment, 100 parts of arsenic acid were combined with 131·2 parts of barytes.

Now 132·7 parts of barytes contain 13·89 parts of oxygen, and 131·2 contain 13·77: so that the analysis of arseniate of lead lies between these two numbers; but we know that arsenic acid contains more than twice that quantity of oxygen. It must, then, contain three times 13·89, or 41·67 per cent. of oxygen.

I thought the most accurate way of verifying this result would be to decompose arsenious acid by means of sulphur in a small apparatus of a determined weight. By finding the weight of the sulphurous acid gas disengaged, it would be easy to infer the quantity of oxygen in arsenious acid. I mixed 5 parts of arsenious acid with 20 parts of sulphur in a small retort, to the neck of which I had fitted a narrow glass tube 36 inches in length, to prevent any of the sulphur from being mechanically carried along with the sulphurous acid gas. I heated the retort till the disengagement of sulphurous acid gas was at an end, and the sulphuret of arsenic began to sublime. The apparatus had lost 3·05 parts of its weight by the disengagement of the sulphurous acid gas, in which there ought to be 1·5185 part of oxygen. Hence 100 parts of arsenious acid contain at least 30·37 parts of oxygen, and not, as has been hitherto believed, only 25 parts. Hence if arsenic acid contains 1½ times as much oxygen as arsenious acid, it follows that it contains at least 40 per cent. of oxygen. This agrees sufficiently with the analysis of the arseniates. I conceive, however, that more confidence is due to the conclusion drawn from the analysis of the arseniates than from that of arsenious acid.

M. Laugier has published a set of excellent experiments on the composition of some arseniates and arsenical sulphurets. He obtained results very different from those which I have just stated; and one would be disposed to say that an observation which he has made, namely, that in the arseniate of barytes the acid is to the base inversely as it is in the arseniate of lime, is contrary to the laws of chemical proportions, respecting which M. Laugier does not seem to have any idea. We shall see, however, that the observation of Laugier is not without foundation. As we cannot suppose that this skilful chemist could have analysed a neutral arseniate of barytes so badly as to have obtained 33$\frac{1}{3}$ per cent. of barytes instead of 42·9 per cent., I resolved to examine if there existed subarseniates of lead and barytes. I obtained such salts by digesting the neutral arseniates in caustic ammonia, which extracted a portion of the acid and left the subarseniate undissolved: 100 parts of the subarseniate of barytes thus procured and heated to redness yielded 101·4 parts of sulphate of barytes; and 100 parts of subarseniate of lead yielded 101·5 parts of sulphate of lead. Hence the composition of these subarseniates is as follows:—

Arsenic acid	33·5	100
Barytes	66·5	198·59
	100·0	

Arsenic acid	25·25	100
Oxide of lead	74·75	296·4
	100·00	

We find that the acid in these subsalts is neutralized by 1$\frac{1}{2}$ as much base as in the neutral arseniate, and of course the acid contains twice as much oxygen as the base. I must observe that the small aberration in the subarseniate of lead will be accounted for by supposing an error of 0·002 in the weight of the subsalt analysed. We see that Laugier analysed the subarseniate of barytes, because he found that his arseniate when converted into sulphate did not alter its weight. If we admit, according to the above-mentioned observation of M. Laugier, that the arseniate of lime is composed of 66·5 acid and 33·5 of lime, it will follow that he analysed the neutral combination of that earth; for 33·5 parts of lime contain 7·43 of oxygen, which multiplied by 3 = 28·29; and 66·5 of acid contain 27·73 parts of oxygen.

The existence of these subsalts proves that arsenic acid must contain either 3 or 6 volumes of oxygen; but before discussing this point, I must speak of my experiments on arsenious acid.

I dissolved arsenious acid by boiling it in caustic ammonia. I found that, though the ammonia was perfectly saturated, it was disengaged by continuing the heat while at the same time small white crystals were deposited on the sides of the vessel. I found that these were crystals of arsenious acid quite free from ammonia. Hence it follows that the ammonia was completely saturated. This solution well corked was exposed for several days to the freezing temperature of water to get rid of the arsenious acid held in solution by the water. The neutral ammoniacal solution was then employed to precipitate a solution of 10 parts of nitrate of lead. The mixture when heated deposited a white heavy matter, which being washed and fused in a glass retort weighed 12·27 parts. It produced a transparent somewhat yellowish glass. The residual liquor being treated with sulphate of ammonia deposited 0·357 part of sulphate of lead, equivalent to 0·262 of oxide of lead. We must subtract these 0·262 from the 6·731 contained in the nitrate of lead, to obtain the quantity of oxide of lead contained in the 12·27 of arsenite of lead; that is to say, 6·47 of oxide of lead were combined with 5·82 of arsenious acid. Hence the salt is composed of

Arsenious acid	47·356	100·00
Oxide of lead	52·644	111·17
	100·000	

Now these 111·17 parts of oxide of lead contain 7·95 of oxygen, which multiplied by 4 = 31·8; so that the arsenious acid contains four times as much oxygen as the base which it saturates.

In my former experiments with arsenite of lead, I found 100 of arsenious acid combined with 118·9 of oxide of lead, which induced me to believe that the arsenious acid neutralized a quantity of base containing $\frac{1}{3}$ of the oxygen of that acid. We see that with respect to the composition of this arsenite my former experiments agree sufficiently with the present ones, though neither of them probably are quite exact: but as to the consequence to be drawn from them respecting the composition of arsenious acid, it is clearly different from what I formerly drew.

To see whether arsenious acid be capable of forming subsalts, I prepared a subacetate of lead as free as possible from neutral acetate and from the subacetate at a maximum. I precipitated a solution of this subacetate by means of arsenite of ammonia, taking care not to precipitate the whole of the lead. This precipitate being well washed and fused, I dissolved 10 parts of it in nitric acid, and decomposed it by sulphuric acid with the precautions mentioned above. I obtained 9·32 parts of sulphate of lead. Hence this subarsenite is composed of

Arsenious acid	31·3	100·0
Oxide of lead	68·7	219·5
	100·0	

It is easy to see (abstracting a little inaccuracy in the experiment) that the acid in this salt is combined with twice as much base as in the neutral arsenite, and therefore must contain twice as much oxygen as the oxide with which it is combined.

I made some experiments, likewise, with the arsenites of barytes; but they presented exactly the same difficulties as the borates of barytes.

From the preceding experiments the two acids of arsenic ought to be composed as follows:—

1. *Arsenious Acid.*

Arsenic from 67·75 to 68....100
Oxygen from 32·25 to 32.... 47·626 to 46·926

2. *Arsenic Acid.*

Arsenic 58·33 to 58·7....100
Oxygen 41·67 to 41·3.... 71·439 to 70·4

These determinations in maximum and minimum are calculated from the two analyses of neutral arseniate of barytes.

It is evident that the number of volumes of oxygen contained in these acids must be either 2 : 3 or 4 : 6. The composition of the neutral arsenites speaks very decidedly in favour of the last numbers; but that of the neutral arseniates seems favourable to the first: and so much the more, because, except the suboxide of arsenic, we know no other oxides of that metal than the two acids: but, on the other hand, the composition of the subarseniates does not agree well with the supposition that arsenic acid is $As + 3 O$; for in that case (since the oxide of lead is $P + 2 O$) the subarseniate of lead would be $P \overset{2}{O} + 1\frac{1}{3} As \overset{3}{O}$. Now the fraction $\frac{1}{3}$ is nowhere else to be found in chemical compounds: but if arsenic acid be $As + 6 O$, the subarseniate in question will be $As \overset{6}{O} + 1\frac{1}{2} P O$. These considerations alone would induce me to admit six volumes of oxygen in arsenic acid; but there are other proofs, more convincing, of which I shall now give an account.

M. Laugier has found that realgar, or native sulphuret of arsenic, when oxidated by nitromuriatic acid, and treated with muriate of barytes, yields from 300 to 304 per cent. of sulphate of barytes. As sulphate of barytes contains 13·76 per cent. of sulphur, this quantity is equivalent to 41·28 or 41·83 per cent. of sulphur in the sulphuret of arsenic. Hence 100 parts of arsenic combine with 71·3 or 71·89 of sulphur: but this quantity is equal to the oxygen in arsenic acid. Hence this sulphuret is proportional to a degree of oxidation of arsenic which contains half as much oxygen as the acid in *ic*: but this oxide is unknown. If it exist, arsenic acid must contain 6 volumes of oxygen; otherwise this new oxide would be $As + 1\frac{1}{2} O$.

As I was not in possession of any native realgar, I endeavoured to prepare it artificially by distilling sulphur with arsenious acid. I mixed 4 parts of sulphur with 1 part of arsenious acid; and when the disengagement of sulphurous acid gas was at an end, and a portion of the sulphur sublimed, I allowed the retort to cool. I found two different layers: the upper one was thin, yellow, and opake, and was pure sulphur; the lower one was pellucid, had a brownish yellow colour, and was exactly similar to Burgundy pitch. To see whether it was any thing else than a mixture of sulphur and sulphuret, I fused it again with sulphur, mixing them well together; but the two liquids again separated. The sulphuret being the heaviest sank to the bottom, and the sulphur floated on the surface. This substance was, then, a supersulphuret of arsenic at the true maximum: 5 parts of this supersulphuret oxidated by nitromuriatic acid yielded 26·72 parts of sulphate of barytes, equivalent to 3·6767 of sulphur: so that this sulphuret is composed of

Arsenic	26·47	100
Sulphur	73·53	280
	100·00	

This sulphuret contains four times as much sulphur as realgar; for 71·5 × 4 = 286: and if realgar, as we shall see afterwards, is composed of As + 3 S, the other sulphuret must be As + 12 S, that is to say, the maximum of combination admitted by the hypothesis of atoms. This supersulphuret gives a beautiful yellow colour, which might be employed as a paint, and could be fabricated at a moderate expense.

This supersulphuret not being artificial realgar, as I had at first supposed, I endeavoured to procure realgar from it by distilling it in a retort. I obtained at first a little sulphur, then supersulphuret very little coloured, and afterwards portions which became more and more red as the distillation advanced. When there remained but very little sulphuret I allowed the retort to cool. I divided the sublimate according to its colour into four different portions, which I oxidated by the nitromuriatic acid. None of these portions was composed as I expected. I found them all mixtures of different degrees of sulphuration. The portion of sublimate nearest the belly of the retort was composed of 100 arsenic and 84·3 of sulphur. The ruby red and very brilliant drops which had condensed in the retort itself contained 76 sulphur to 100 of metal: so that the portions sublimed, in proportion as the distillation went on, approached more and more to realgar. I now mixed one part of the last sulphuret obtained with metallic arsenic, and sublimed the mixture in a long-necked phial. But I found the metal and sulphuret so nearly of the same volatility, that the sulphuret was mechanically mixed with crystals of metallic arsenic. The sulphuret did not appear altered. Hence it follows that there does not exist a sulphuret containing less sulphur than realgar, at least it cannot be formed by means of heat. These experiments are sufficient to show that the analysis of realgar by Laugier must be exact. Of course this sulphuret is proportional to an oxide of arsenic still unknown.

In my former experiments I had already suspected the existence of such an oxide, and I endeavoured to obtain it combined with muriatic acid by distilling together muriate of lead and metallic arsenic; but as the muriate of lead remained undecomposed, the oxide in question remained undiscovered. Having now such good reasons for believing in the existence of this oxide, I made new attempts to obtain it. I put metallic arsenic into a glass retort, which I exhausted of air, and then filled it with muriatic acid gas previously dried over mercury by means of muriate of lime. I then heated the arsenic by means of a spirit of wine lamp. The arsenic did not lose its metallic lustre; but the upper part of the retort became coated with a flea-coloured crust, not metallic in its appearance, and at first to a certain degree pellucid. The heat having been continued for some minutes, the inside of this coating was all covered with metallic arsenic. When the retort was cooled, I expelled the muriatic acid gas, and filled the retort with common air. I could not discover by the smell that the muriatic acid gas was mixed with arsenical hydrogen gas. I poured water into the crucible, but the brown coating was not altered. On adding a little caustic potash ley, the crust detached itself from the retort, and was immediately converted into brilliant flocks of metallic arsenic.

As this experiment was not conclusive, probably because the water of muriatic acid does not readily allow itself to be decomposed by the metal, I substituted calomel in place of the muriatic acid gas. With this I mixed pounded arsenic, and distilled the mixture in a glass retort furnished with a receiver. I obtained some drops of a white and smoking liquid, which, when mixed with a little alkali, deposited arsenious acid: it was, therefore, *acidum muriatico-arsenicosum*. In the neck of the retort there was deposited a red or brownish red sublimate, which formed a tube, the inside of which was coated with an amalgam of arsenic in a half liquid state. The red sublimate gave a yellow powder: it was entirely insoluble in water. Mixed with muriatic acid, and placed upon polished copper, it gave no mercurial stain, as common muriate of mercury does.

As this sublimate contained evidently portions of calomel upon which the arsenic had not acted, I mixed it carefully with a new portion of metallic arsenic, and sublimed it a second time. What sublimed was at first of a fine red colour, and perfectly transparent. Afterwards it became deeper coloured and opaque. It was then easily detached from the retort, and constituted a brown mass bordering upon yellow, without a crystalline fracture, and insoluble in water, as before. Mixed with iron, and heated, it gave out a strong smell of arsenic, and was converted into muriate of iron. Hence it obviously contained muriate of arsenic. To obtain the oxide of arsenic, I mixed the muriate with caustic potash. It assumed a grey colour, and a metallic brilliancy; and after some hours it was converted into an amalgam, in which pieces of metallic arsenic floated. The liquor contained muriate and arsenite of potash. Ammonia, the alkaline carbonates, and in general all substances which combined with muriatic acid, decomposed it in the same manner, though less rapidly.

It follows that the brown sublimate is a triple muriate with a mercurial base, and that the arsenic has a lower degree of oxidation than arsenious acid, because when it forms arsenious acid by the action of an alkali, not only a part of itself is reduced to the metallic state, but likewise the whole quantity of oxide of mercury present.

All my attempts to convert this triple muriate into pure muriate of arsenic have been ineffectual. It follows from these experiments that arsenic, among other properties which it has in common with sulphur, has this also, of producing a salifiable oxide with muriatic acid, but which, like the oxides of sulphur, has no existence in a separate state, but the instant it is separated from muriatic acid is decomposed into metallic arsenic and arsenious acid. Though I have not yet made analytical experiments on this muriate, there can be no doubt that it is proportional to the sulphuret at a minimum, that is to say, that it must contain half as much oxygen as the acid.

If we add the existence of this oxide to the composition of realgar, it appears to follow clearly that arsenic acid contains 6 volumes of oxygen. Hence a volume of arsenic will weigh 839·9, or at a maximum 852·2.

The known degrees of oxidation of arsenic are, 1. The suboxide or black powder which forms upon metallic arsenic. In my former experiments I found that in it 100 parts of arsenic were united to 8·5 of oxygen. This is certainly either too much or too little, because this suboxide must either be $2 \text{ As} + \text{O}$ or $\text{As} + \text{O}$. I shall make experiments on it hereafter. 2. The salifiable oxide $\text{As} + 3 \text{ O}$. 3. Arsenious acid, $\text{As} + 4 \text{ O}$. 4. Arsenic acid, $\text{As} + 6 \text{ O}$. Is there an oxide $\text{As} + 2 \text{ O}$?

2. *Molybdenum* (Mo).—The degrees of oxidation of this metal have been carefully examined by M. Bucholz. He found that 100 parts of native sulphuret of molybdenum gave from 288 to 290 parts of sulphate of barytes, and that it contains about 1 per cent. of foreign matters. According to this determination, 100 of molybdenum combine with 66·5 of sulphur. M. Bucholz found, likewise, that 100 parts of the sulphuret of this metal gave 90 parts of molybdic acid. In other experiments in which he oxidized metallic molybdenum, he found that 100 of the metal combined with from 49 to 50 of oxygen to form molybdic acid. These experiments accord well with each other: but the composition of the sulphuret is not proportional to that of the acid, which according to Bucholz was the only oxide of the metal known. But that eminent chemist discovered that molybdenum forms likewise an acid in *ous*, and a suboxide of a very dark purple colour. It seems to follow from the

proportions prescribed by Bucholz for the preparation of molybdous acid, that this acid is proportional to the sulphuret of molybdenum, that is to say, Mo + 2 O, while the molybdic acid is composed of Mo + 3 O.

But to assure myself of the accuracy of this determination, I thought it requisite to examine the capacity of saturation of molybdic acid. Having made some vain attempts to analyse molybdate of lead and molybdate of barytes, I found that the only method of obtaining an exact result was to form molybdate of lead. I dissolved 10 parts of neutral nitrate of lead in water, and poured an excess of molybdate of ammonia into the liquid (the molybdate was crystallized in a mother water strongly alkaline; of course it was neutral. This deserves notice, because there is a supermolybdate of ammonia which is always formed when we attempt to concentrate a solution of neutral molybdate by evaporation). The molybdate of lead washed, dried, and heated to redness, weighed 11·068. The liquid from which it had been precipitated did not, when mixed with sulphate of ammonia, exhibit any traces of lead; therefore these 11·068 of molybdate of lead contain 67·3 per cent. of oxide of lead. Hence the salt is composed of

Molybdic acid	39·194	100
Oxide of lead	60·806	155·15
	100·000	

The 155·15 of oxide of lead contain 11·093 of oxygen. Now 11·093 × 3 = 33·279. Therefore molybdic acid is composed of

Molybdenum	66·721	100
Oxygen	33·279	49·88
	100·000	

This result is nearly a mean of the experiments of Bucholz. Hence the volume of molybdenum must weigh 601·56; and its suboxide should be Mo + O. I must not conceal, however, that its analogy with arsenic and chromic acids renders it probable that molybdic acid contains 6 volumes of oxygen.

3. *Chromium* (Ch).—Nobody has hitherto made exact experiments on the quantity of oxygen which this metal absorbs in its different degrees of oxidation. M. Vauquelin established only that chromic acid appears to contain about 40 per cent. of oxygen.

I made the following experiments to determine this point. I prepared chromate of lead and chromate of barytes by precipitating a solution of neutral chromate of potash by nitrate of lead and by muriate of barytes.

A. *Chromate of Lead.*—A solution of 10 parts of nitrate of lead precipitated by chromate of potash yielded 9·8772 parts chromate of lead. The residual liquid gave no traces of lead when treated with sulphuric acid: therefore the 9·8772 of chromate of lead contain 6·73 of oxide of lead; so that this chromate is composed of

Chromic acid	31·761	100
Oxide of lead	68·239	213·841
	100·000	

Now 213·841 of oxide of lead contain 15·29 of oxygen. Hence chromic acid must contain 2, 3, or 4 times that quantity of oxygen.

Ten parts of native chromate of lead (in picked crystals) treated with a mixture of alcohol and muriatic acid, were almost immediately decomposed, with the disengagement of heat, and the production of ether. The muriate of lead remained undissolved, while the muriate of chromium dissolved in the spirituous liquor. The liquor being evaporated nearly to dryness to get rid of the excess of acid, I mixed the residue with alcohol to dissolve the muriate of chromium. The muriate of lead, being well washed with alcohol, was dissolved in water, and left undissolved 0·1 of foreign matter. I evaporated the solution of muriate of lead in a platinum crucible, exactly weighed, and I dried the muriate on a sand-bath at a high temperature: I obtained 8·435 parts of muriate of lead. The solution of muriate of chromium precipitated by ammonia gave green oxide of chromium, which, when heated to redness, weighed 2·388. The residual ammoniacal liquid being evaporated to dryness and calcined, left 0·013 of green oxide of chromium. Now as 99 parts of chromate of lead produced 84·35 of muriate of lead, which contains 80·3876 per cent. of oxide of lead (*Ann. de Chim.* Aug. 1811, p. 136), it follows that the chromate ought to be composed of

Oxide of lead	68·50
Green oxide of chromium	24·14
Loss = oxygen of the acid	7·36
	100·00

This analysis does not differ from the preceding synthesis but ¼ per cent., and may therefore be considered as pretty exact. It follows that 31·5 of chromic acid are composed of 24·14 of green oxide and 7·36 of oxygen: but the 68·5 of oxide of lead contain 4·8997 of oxygen, which is not a submultiple of 7·36 by a whole number: but 7·36 is exactly 1½ times the oxygen in the oxide of lead; for 4·8997 × 1½ = 7·3465. Hence we see that the acid had lost 1½ times as much oxygen as the base contains.

B. *Chromate of Barytes.*—This fact required to be verified by another experiment. I therefore treated 10 parts of chromate of barytes previously heated to redness with a mixture of muriatic acid and alcohol. I then separated the barytes by means of sulphuric acid. I obtained 9·1233 parts of sulphate of barytes; therefore chromate of barytes is composed of

Chromic acid	40·15	100·000
Barytes	59·85	149·066*
	100·00	

Now 149·066 of barytes contain 15·6 of oxygen, which differs a little from what we found by the synthesis of chromate of lead; but this is owing to the property which sulphate of barytes has to precipitate along with it a portion of the oxide of chromium: and this happens in almost all the metalline solutions from which it is precipitated; as, for example, those of iron and copper. The solution of green muriate of chromium, from which the sulphate of barytes was precipitated, was now evaporated to dryness in a platinum crucible, and exposed to a red heat. It left 3·043 of green oxide of chromium. Thus the chromate of barytes, besides the 59·85 parts of barytes, has given 30·43 of green oxide of chromium and 9·72 of loss, which ought to be the oxygen of the decomposed acid; but 59·85 of barytes contain 6·284 of oxygen, which multiplied by 1½ = 9·426. We see, then, that abstracting from the imperfection of the experiment, the acid has lost, in becoming green oxide, 1½ times as much oxygen as the barytes contains.

This seems to prove that chromic acid contains twice as much oxygen as the green oxide, and 3 times as much as the base by which it is neutralized; for if the green oxide contained twice as much oxygen as it required to become acid, the oxygen of the acid could not be a multiple by a whole number of the oxygen in the base by which it is neutralized: and, on the other hand, if the green oxide contained 3 times as much oxygen as would be necessary to convert it into acid, the quantity of oxygen in that oxide would surpass the bounds of credibility. The green oxide, then, can only contain a quantity of oxygen equal to that necessary for converting it into the acid, or half that quantity. In the first case the acid contains 3 times as much oxygen as the base by which it is neutralized; and in the second case, twice as much.

To determine between these two probabilities, I prepared a green muriate of chromium, which I evaporated to dryness to get rid of the excess of acid. A solution of this salt was precipitated by a great excess of ammonia, and the solution being filtered and neutralized by nitric acid, I precipitated it by nitrate of silver. I obtained 30·5 parts of green oxide of chromium and 156·1 of muriate of silver, equivalent to 29·73 of muriatic acid. Now 29·73 : 30·5 :: 100 : 102·4; but in these 102·4 of green oxide the muriatic acid supposes 29·454 of oxygen: therefore 100 parts of green oxide must contain 28·7 parts of oxygen: but if, as we have determined above, the oxide of chromium contains a quantity of oxygen equal to that necessary to convert it into an acid, it ought to contain 29·7 per cent. of oxygen. The above analysis, though it only gives an approximation, proves, however, that in chromic acid the metal is combined with twice as much oxygen as in the green oxide. If we take the synthesis of chromate of lead as the most exact experiment of those which I have given, and if we calculate from it the composition of the oxide and acid of chromium, we obtain the following results :—

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* In two other experiments I obtained for 100 of chromic acid 149·2 to 149·5 of barytes. I ascribe this circumstance to the sulphate of barytes being always a little coloured by chromic acid, notwithstanding the excess of acid; but this was scarcely perceptible in the experiment stated in the text.

Green Oxide of Chromium.

Chromium	70·24	100·00
Oxygen	29·76	42·37
	100·00	

Chromic Acid.

Chromium	54·13	100·00
Oxygen	45·87	84·74
	100·00	

Now what is the number of volumes of oxygen contained in each of these oxides ? The composition of the chromates does not allow us to conjecture that chromic acid is either Ch + 2 O or Ch + 4 O ; and as it is not probable that the green oxide is Ch + $1\frac{1}{2}$ O, the acid cannot be Ch + 3 O ; therefore no other nnmber remains but Ch + 6 O.

To throw some further light on this subject, I endeavoured to obtain oxides containing less oxygen than the green oxide ; but could not succeed. I exposed muriate of chromium previously dried in a red heat to a strong heat in a retort, with the view of obtaining oxymuriatic acid and a muriate at a lower degree of oxidation than the green oxide ; but the experiment did not succeed. I obtained at first a little muriatic acid ; then a pale red substance sublimed, in form of small brilliant scales. The greatest part of the substance remained unsublimed, and was slowly soluble in water. The sublimate was insoluble, and appeared to be a submuriate of green oxide.

M. Vauquelin discovered some time ago a new oxide of chromium, which, according to him, is intermediate between the green oxide and the acid. He obtained that oxide by heating the nitrate of chromium. It is obvious that the existence of this oxide can only be explained by supposing the acid to contain 6 volumes of oxygen. I resolved, therefore, to verify it. I dissolved in nitric acid a portion of hydrous green oxide, and evaporated the solution to dryness. The dried nitrate being slightly heated, swelled, assumed a brown colour, and allowed nitrous acid to escape. I removed a small portion at this part of the process, and dissolved it in water. The solution had a brownish red colour, very different from that of chromic acid. Caustic ammonia precipitated from this solution bulky brown flocks. The oxide thus obtained dissolved readily in sulphuric acid, forming a deep brown solution, which, after being some time exposed to the light, became green. The other portion of the nitrate of chromium was heated on a sand-bath till it gave out no more nitrous vapours. I then poured more nitrous acid over the brown mass, and continued the evaporation. When it ceased to give out nitrous vapours I allowed it to cool. It was now deep brown, rather brilliant, and mostly insoluble in water and alkalies. What the water dissolved was only a portion of brown nitrate not decomposed. The brown oxide thus obtained was insoluble in acids; but muriatic acid decomposed it with disengagement of oxymuriatic gas. This intermediate oxide, then, exists, and shows us that the acid must contain either 4 or 6 volumes of oxygen; but as we have seen from the composition of the chromates that the acid cannot contain 4 volumes, it must of necessity contain 6 volumes. Hence the known oxides of chromium are, 1, Green oxide (oxidum chromosum), Ch + 3 O. 2. Brown oxide (oxidum chromicum), Ch + 4 O. 3. Chromic acid, Ch + 6 O. The volume of chromium, then, must weigh 708·045.

But before quitting my experiments on chromium, I think I ought to state the following, though it has nothing to do with the principal object of this memoir. The green oxide of chromium undergoes combustion at a certain temperature, just as is the case with several metallic stibiates and stibiites. When the hydrous green oxide of chromium is heated to a cherry red, it loses its water, and becomes deep green, almost black. If we now weigh it, and then expose it to a strong heat, it appears to take fire, and to burn an instant or two with a great deal of intensity. If we weigh it after it has cooled, we find that its weight is not altered, but its colour has become a very beautiful light green, and it is perfectly insoluble in acids. If we make this experiment with a hydrate containing a small portion of subsulphate or subnitrate of chromium, the acid remains combined with the oxide till the moment of incandescence, when it is disengaged, and constitutes a little smoke, and in this case the oxide loses a little of its weight; but even in experiments when this smoke was very perceptible the oxide did not lose more than $\frac{1}{4}$ or $\frac{1}{3}$ of its weight by the ignition. I wish to draw the attention of the reader to this circumstance, that when the phenomenon takes place in the stibiates it consists in the action of compound bodies (the oxide of antimony and the base), while in the present case it consists in the action of elementary bodies (chromium and oxygen). Sir Humphry Davy has observed an analogous phenomenon in the hydrate of zirconia, and he ascribes it to the increased cohesion of the parts of the earth at the instant that the water separates from them. According to Mr. Edmund Davy, the precipitate obtained from the muriate of platinum by sulphureted hydrogen produces the same phenomenon, if, after being dried in an atmosphere destitute of oxygen, it be heated in a retort exhausted of air. It emits a little sulphurous acid gas and a little sulphureted hydrogen, and exhibits, at the same time, the phenomenon of taking fire. It leaves for residue sulphuret of platinum. It is evident that the phenomenon in this case must be referred to the more intimate combination between the sulphur and platinum in the sulphuret, than in the hydrosulphuret. We shall see hereafter that the same phenomenon takes place with one of the oxides of rhodium, which exhibits the appearance of combustion the instant that a portion of its oxygen is disengaged, and it is reduced to a suboxide. I shall mention the circumstance more particularly below.

(*To be continued.*)

Article IV.

On Iodine, the Violet-coloured Substance mentioned in the last Number of the Annals of Philosophy.[*]

The new substance discovered by M. Courtois, noticed in a preceding Moniteur, has been subjected to examination by M. Gay-Lussac, at the request of his friend M. Clement. We shall state at present the principal results which he obtained.

The new substance to which the name of *iodine* (from ιωδης, *violaceus*,) may be given, possesses in a high degree the electrical properties of oxygen and oxymuriatic acid. When it has been purified, by means of potash and distillation, it is infusible at the temperature of boiling water, and possesses nearly the volatility of that liquid. No re-agent discovers in it the smallest trace of muriatic acid.

Iodine combines with almost all the metals; but, as it is solid, it does not appear to disengage so much heat in these combinations as oxymuriatic acid does, though in its general properties it has a great resemblance to this body. To give an idea of its relation to other substances, we shall compare it with that acid, applying to it the two hypotheses that have been devised respecting its nature, and we will add that by combining with hydrogen it forms a particular acid, very energetic, capable of assuming the gaseous form, very soluble in water, and bearing the same relation to iodine that muriatic acid does to chlorine. The action of phosphorus on iodine furnishing the means of obtaining the new acid in its gaseous and liquid state, we shall begin by describing it.

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[*] This curious paper is translated from the Moniteur for the 12th of December, and appears to have been drawn up by M. Gay-Lussac. It will give the chemical reader a much more correct view of this extraordinary substance than the very imperfect and inaccurate notice in our last Number. A paper by Sir H. Davy on the same substance has been sent to Sir Joseph Banks. He enters into the requisite details respecting its preparation and properties. As soon as it is read before the Royal Society it will make us better acquainted with this new supporter of combustion. These supporters are now three; and supposing Davy's fluorine to exist, they amount to four. How much these new discoveries must alter the presently received chemical theory, and how they serve to confirm Davy's opinions respecting muriatic acid, is too obvious to escape observation.—T.

If we bring dry iodine and phosphorus in contact, we obtain a matter of a reddish brown colour, and no gas is disengaged. If we moisten this matter, it gives out immediately acid fumes in abundance, while at the same time phosphorous acid is formed. We easily obtain the new acid in the gaseous state by employing iodine a little moistened. There is then sufficient water to occasion its formation, but not to condense it. If we combine the phosphorus and iodine under water only a little subphosphureted hydrogen gas is disengaged, and the water becomes very acid. If the new substance is in excess, the liquid is strongly coloured reddish brown; but it is colourless when the phosphorus exceeds. There commonly remains a mass of a red colour, which refuses to dissolve in water, and in which both phosphorus and iodine may be found; but the proportions of the two ingredients may be such that the whole dissolves in water without any residue, and the liquid in that case is limpid, like water.

If we distil the acid liquid, the water at first is disengaged, and the new acid does not pass into the receiver till the liquid in the retort becomes very concentrated: at last nothing remains in the retort but phosphorous acid, which soon disengages abundance of phosphureted hydrogen gas. Thus it appears that, when the phosphorus and iodine are dry, there is formed a combination analogous to that of oxymuriatic acid and phosphorus; and when they are moist the same thing takes place as when the liquid of phosphorus is thrown into water. Hence it would appear that while the oxygen of the water unites with the phosphorus and forms phosphorous acid, its hydrogen combines with the iodine and forms the new acid.

The following are the characters of this new acid. In the gaseous state it is colourless, has nearly the smell of muriatic acid, smokes when in contact with air, is rapidly absorbed by water, gives with oxymuriatic gas a beautiful purple vapour, and is rapidly altered when allowed to remain over mercury. With this metal it forms a greenish yellow substance, similar to that which is obtained directly with mercury and the vapour of *iodine*, while a quantity of hydrogen gas is evolved equal in volume to one half the volume of the acid gas. A few minutes' agitation is sufficient for the entire decomposition. Iron and zinc produce a similar effect.

This acid in the liquid state, obtained by dissolving the gas in water, forms a very dense liquid, and not very volatile. It rapidly decomposes the alkaline carbonates, dissolves iron and zinc, with the disengagement of hydrogen gas; but does not attack mercury, even when assisted by heat; a proof of the strong affinity which it has for water. It forms with barytes a soluble salt, and gives with corrosive sublimate a red precipitate, soluble in an excess of the acid. When a few drops of oxymuriatic acid are added to it the substance is immediately regenerated. Heated with the black oxide of manganese, with red lead, or the brown oxide of lead, iodine is disengaged, and the oxides are brought into a state of solubility in acids. The red oxide of mercury does not regenerate iodine. We may conclude that all the oxides capable of changing muriatic acid into chlorine will convert the new acid into iodine. When the new acid is dissolved in water, and subjected to the action of the galvanic pile, iodine makes its appearance at the positive pole. When this acid has once entered into a combination, it is not easily disengaged again; for example, when sulphuric acid is brought into contact with the combination of the new acid and potash, sulphurous acid is formed and iodine is disengaged. Nitric acid in the same way furnishes nitrous acid. If the phosphoric or boracic acid be employed, either dry, or in solution in water, no decomposition takes place.

It is easy to conceive, from what has been stated, what happens when iodine is placed in contact with other bodies.

With hydrogen at a low or high temperature the acid is obtained: but the acid is usually impure, because it has the property of dissolving a great quantity of iodine, and of defending it from the contact of the hydrogen.

Sulphureted hydrogen speedily deprives iodine of its colour, and converts it to an acid, while sulphur is deposited. It produces the same effect, when the new substance is in combination with the alkalies, forming brown and colourless solutions. It is to be observed that, when a solution of iodine in ether or alcohol is precipitated by sulphureted hydrogen, no sulphur is deposited.

Sulphurous acid speedily converts iodine into an acid, passing itself to the state of sulphuric acid. Phosphorous acid and the sulphureted sulphites likewise produce the same acid. Hence we may conclude that in kelp, which contains a good deal of sulphureted sulphite, the new substance is in the state of an acid. It does not appear in the mother ley of that substance till the sulphureted sulphites are destroyed.

Iodine is not altered by charcoal and sulphurous acid, because these substances cannot furnish it with hydrogen to convert it into an acid. It does not decompose water at any temperature. It deprives indigo of its colour. It is separated from its combinations by the mineral acids, and even by acetic acid. It combines with most of the metals without disengaging any gas. When some of these combinations are made to take place under water, as, for example, the combination of iodine and zinc, nothing is disengaged. The liquor, at first strongly coloured, becomes by degrees as limpid as water. The alkalies precipitate from it a matter which has all the characters of oxide of zinc, but which retains a little of the new acid. Here the water has been decomposed, and oxide of zinc and the new acid produced. This compound, like all those that contain the new acid, gives out sulphurous acid when treated with sulphuric acid. 18 parts of iodine dissolve about 3½ of zinc. Hence we may conclude that the ratio in weight of oxygen to iodine is as 1 to 20, or 15 to 300. With oxymuriatic acid it forms an orange yellow substance, crystalline, volatile, deliquescent, and appearing to exist with two different proportions of iodine.

Iodine forms, as is known, a fulminating powder with ammonia; but the theory is very simple if we consider that iodine has a great tendency to combine with hydrogen.

From the preceding statement we cannot but perceive the analogy between iodine and chlorine, the new acid and muriatic acid. It is very remarkable that hydrogen is always necessary to convert iodine into an acid. This substance appears to act the same part with respect to a certain class of bodies as oxygen does to another. All the phenomena, of which we have spoken, may be explained by supposing iodine to be an element, and to form an acid by combining with hydrogen; or by supposing that this last acid is a compound of water and an unknown base, and iodine this base united to oxygen. The first hypothesis appears to us, from the preceding facts, more probable than the last; and it serves, at the same time, to give more probability to that which considers oxymuriatic acid as a simple body. If we adopt it, the name which seems most suitable to the new acid is *hydriodic acid.*

ARTICLE V.

Answer to Dr. Grierson's Observations on Transition Rocks. By Thomas Allan, Esq. F.R.S.E.

(To Dr. Thomson.)

SIR,

When men of science have devoted themselves to any particular study, whether for profit or amusement, it is very natural that they should feel a desire of rendering their labours useful to society; and it sometimes may happen that such a motive is the only inducement that tempts an individual to depart from the privacy of his study, and submit himself to the indiscriminating censure of the public; yet how often do we see the critic unjustly garbling and interpolating the work of an unfortunate author, and frequently mis-stating facts, merely to give weight to his own argument; hoping that few of those to whom his acrimony is addressed will take the trouble to investigate whether he be in the right or not; and at any rate, that his words, as being the last, will make some impression on a great proportion of those who may chance to peruse them.

From what I understand to be the character of the gentleman who has taken so much trouble in your Number for August to expose the futility of my observations on the transition rocks of Werner, I am persuaded he did not allow himself to wander so much out of the track in which I am told his friends have been in the habit of finding him, merely for the purpose of indulging in observations, which are delivered in a way so unsuitable to the estimation in which he is held by them; and as some of these had long before reached me from another quarter, I am satisfied that the sentiments contained in his paper are not all, originally at least, his own. Besides, the circumstance of your having been pressed by some gentlemen in this place to insert my paper, and your doing so without giving me the slightest intimation, is a clear proof to me that Dr. Grierson is not the only one who was engaged in drawing up the communication which bears his name.*

Your having inserted that paper, and thereby paved the way for Dr. Grierson's communication, afforded me an opportunity which might have been of extreme utility, as it conveyed some very pointed criticisms on a paper which is not even yet published, and which I most certainly would have altered, had I found on examination that any part of it had in the slightest degree been shaken by his observations, or that I had committed any of those mistakes so lavishly attributed to me. This, however, I found no occasion to do, and it was my intention to have overlooked Dr. Grierson entirely, had you not done me the favour to notice the same subject in your Numbers for October and November.

To you I am greatly obliged for the confirmation you have afforded some of my opinions. But, in the first place, I shall discuss Dr. Grierson's observations.

The object of this gentleman's paper has been to throw discredit upon mine, by charging me with wilful misconception, with ignorance, and presumption; language which will produce a very opposite effect from that intended by the writer. His style, however, it is not my intention to comment upon.

It has generally been observed, that it is much easier to find defects in a system than to invent a better one. The reason is evident, because if it contains any incontrovertible points, it is sufficient to overlook them and consign them to oblivion, by descanting solely on their weaker neighbours, and trying to make it be believed, that the merits of the writer have been fully considered in the acute arguments of the critic. This, although a most common practice, is certainly not fair, nor should it ever be resorted to, where a person wishes to have a subject fairly and candidly discussed.

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* I think it necessary to set Mr. Allan right with regard to this statement, which is not quite correct. I received the paper in question from Mr. Allan, enclosed in a blank cover. As I had been in the habit myself of sending printed papers in that way to the London Journals, and as they had always been immediately printed, I took it for granted that Mr. Allan's object in sending his paper was to have it inserted in the *Annals of Philosophy*. At the same time I was rather unwilling to print it, because I foresaw that it would lead to a controversy, which I was anxious to avoid. On that account I mentioned the circumstance to several gentlemen, in order to obtain their opinion. Among others, I mentioned it to Mr. Greenough and Mr. Robert Brown. Mr. Greenough advised me not to print the paper, for reasons which it is not necessary to mention. All the other gentlemen told me that, if I withheld it, Mr. Allan would accuse me of partiality, and of excluding all geological papers written on a certain side. This opinion determined me to insert the paper in the next Number of the *Annals of Philosophy*. I consulted on the subject two gentlemen in Edinburgh; but neither Mr. Jameson nor Dr. Grierson was of the number. As Mr. Jameson was directly, and I thought indecorously, attacked in Mr. Allan's paper, I was aware that he would and indeed could give me only one advice on the subject, namely, to print the paper. At the time the paper was published I did not know that Dr. Grierson had turned his attention to geological subjects, and therefore did not think of consulting him.—T.

Dr. Grierson seems to be offended at my stating that the conclusions of Werner are more general than were warranted from the circumscribed field to which he was confined; and states that the arrangement of that philosopher, *provided the phenomena of nature are conformable to his views*, is not to be rejected merely because he never travelled beyond the boundaries of Germany. Surely Dr. G. ought to have found out before now that the facts contained in this very *provision* is what I contend against; and he ought likewise to know, that the phenomena of nature are in many respects totally and entirely irreconcileable to the theory of Werner; so much so, indeed, that his own master, the pupil of Werner, has, as a sacrifice due to common sense, been compelled to introduce many alterations in the system he was taught at Freyberg.

Dr. Grierson then proceeds to draw a comparison between the merits of Dr. Hutton and Professor Werner; a comparison quite uncalled for by any thing that appeared in my paper. But if he wishes to know my opinion of these two great men, I will frankly tell him, that as a mineralogist or a practical geologist, Hutton was by many degrees inferior to Werner; but as a philosopher, he was infinitely his superior,*—the hypotheses of the one being founded on observation and pure philosophic induction; while those of the other have originated in a clumsy contrivance, totally unauthorised by all the great features in nature, from which alone we can possibly draw legitimate conclusions.

Dr. Grierson feels displeased that I should have touched upon the "fetters" by which the pupils of his favourite school seem to be spell-bound. He will not, however, persuade me, that any thing else can reasonably account for a man gravely teaching the aqueous formation of *pumice* and *obsidian*.

He next attempts to make it appear that I am very ignorant of the subject I have undertaken to discuss. He states, that because I knew tin and wolfram occurred in Cornwall, *therefore* the granite of Cornwall must be the oldest granite. It will occur, I think, to any other reader, that that circumstance is not the only one that led me to this conclusion, and that I gave it only as a collateral evidence, drawn from the writings of Professor Jameson, in support of other authority which the Doctor found it convenient to pass over in silence, leaving his reader to conjecture that none else existed. He then proceeds to state what Mr. Jameson says with respect to tin and wolfram, as follows:—

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* I wish Mr. Allan had stated here what he means by *philosopher*. As far as the inductive philosophy of Bacon is concerned, the term cannot be applied to Hutton's speculations at all. He began by forming an hypothesis, and then consulted nature merely to obtain proofs of his opinions. Werner's conclusions are all inductions from observation. They can be correct only as far as Werner had an opportunity of making correct observations.—T.

"Let us see what Professor Jameson, in his Elements of Geognosy (the work which Mr. Allan constantly refers to), says with respect to the occurrence of tin and wolfram. In treating of tin, he tells us that it occurs in *very old veins* that traverse granite, gneiss, mica slate, and clay slate; that it occurs disseminated through granite, and in *beds* that alternate with that rock. He adds, that the granite appears to belong to the *newest* formation. (Elements of Geognosy, p. 261.) At p. 309 of the same work, in the tabular view, the Professor gives us again the geognostic situation of tin, and the only granite mentioned is the newest. Of wolfram, he says, at p. 261, that it occurs in veins both in primitive and transition mountains. And again, at p. 311, in the tabular view, wolfram is stated as occurring not in the oldest, but only in the newest granite formation. What shall we think, then, of Mr. Allan's accuracy, when he maintains that, according to the Wernerian geognosy, the granite of Cornwall must be referred to the first or oldest formation because it contains tin and wolfram? But his inaccuracies, or mistatements, do not stop here."

This is as dexterous a quotation as could well have been made; but perhaps it would have been better for the friend of Dr. Grierson that he had let it alone. It is by no means a pleasing task to me to be forced to detect the inconsistencies and contradictions of any one, far less those of a gentleman who I believe has exerted himself as far as he was able to promote the study to which we are equally devoted; but, forced into my present situation, I shall now beg leave to examine what really is said by Professor Jameson on this subject; and I trust I shall make it appear, that I have not consulted his works so very slightly as to be in justice branded with ignorance, or to have quoted them so unfairly as to entitle me to the charge of mistatement.

In p. 275 of his third volume, he states, "molybdena, menachin, *tin*, *scheele*, cerium, tantalum, uran, chrome, and bismuth, are metals of the *oldest primitive formation*, and that only *feeble traces* of them are to be found in newer periods." In p. 261 he states, "that tin occurs in veins traversing granite, *gneiss*, mica slate, and clay slate." Does Dr. Grierson suppose that this granite *connected with gneiss* is that belonging to the newest formation, alluded to in a subsequent paragraph of the same page? Talking of wolfram, in the same page, he states, "it occurs almost always in veins in *primitive* mountains, and sometimes, *though rarely*, in transition mountains;" and afterwards, in so many words, "that its occurrence in grey wacke in the Hartz is *only an exception* to the rule, that wolfram is a very old formation of the primitive period." With respect to Mr. Jameson's tabular view, where he, no doubt, without any comment, *marks* tin as occurring disseminated in newer granite, and wolfram in veins in newer granite, I could consider it in no other light than a mistake, being in direct contradiction not only to what I have above quoted, but what he himself mentions in his second volume, p. 387.* Under the geognostic situation of tin, he states—"It occurs *only in primitive rocks*, as granite, gneiss, &c. *and is the oldest of all the metals*." And p. 490, of wolfram—"It occurs in *primitive* mountains, and in the *oldest* formations. It is almost always accompanied with tin." This I conceive to be quite sufficient authority for the statement I ventured to make respecting the age of these metals, and to warrant me entirely in saying, that as they were considered in other countries indicative of the oldest primitive formations, the same inference must apply to their occurrence in Britain.

Dr. Grierson having, in his own opinion, successfully reduced the age of these two metals to the period of the transition series, now proceeds to subvert my statements respecting the antiquity of the granite itself. In comparing what is stated by Professor Jameson on that subject with the nature of the rock I met with in Cornwall, I found the characters attributed to the oldest granite to coincide exactly with the granite I found in Cornwall; and I have stated, with tolerable distinctness, that such was the general appearance it presented throughout the country. This, however, is the same granite that penetrates in veins the killas of Cornwall, and in doing so is frequently found to contain fragments of the stratified rock. "Who ever heard of fragments being found in the first granite formation?" is the exclamation which escapes Dr. Grierson on the occurrence of this assertion. All I have in answer to say is, that the granite of Cornwall, presenting the characters of the most ancient varieties of that rock, according to the authority of Werner himself, as given by Mr. Jameson, does contain, in the veins which extend from it, abundance of fragments. This fact has subsequently been verified by yourself during your examination of St. Michael's Mount.

Dr. Grierson asserts, that among my other inaccuracies, I have given it as a Wernerian principle, that primitive rocks contain no mechanical deposits; and mentions, as a proof of his position, that according to Jameson conglomerates are found in primitive countries. Now, although a conglomerate should be found in a primitive country, I am not quite certain whether that would constitute a primitive rock. In the second place, although it be quite true that Professor Jameson has mentioned this substance as occurring in his primitive porphyry-suite, I cannot comprehend how a rock formed of fragments *of other rocks* can be supposed to be of *primitive* formation; and, in the third place, the only reference that I have made to this subject is by stating, that from granite down to clay slate no detritus, or any thing like organised bodies, was to be observed; and according to Mr. Jameson (p. 68) I might have gone still farther. Why he afterwards, in p. 101, mentions, that, *excepting* a small proportion of mechanical deposite that accompanies the second porphyry formation; and in p. 351, "that primitive conglomerate forms a bed of inconsiderable magnitude," without stating the reasons for the alteration, I cannot tell.

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* The proper inference would have been that Professor Jameson had altered his opinion; for his third volume was published some years after his second.—T.

The last circumstance I shall here notice is the singular *metamorphosis*, as the Doctor calls it, that takes place upon the grauwacke when it comes in contact with the granite. You have visited St. Michael's Mount, and have found that the clay slate in the neighbourhood of the veins contains so much mica as to have the aspect of mica slate, an alteration which you will observe in all similar situations; and although the Doctor thinks, that because I could *see* no line of division at the Lowran between this altered rock and the common grauwacke, I had no reason to conclude that there was none, I must beg leave to differ from him, and to assure him that I am more inclined to believe my own eyes than any other species of demonstration that can possibly be offered.

Dr. Grierson accuses me of laying stress upon the opinions of the vulgar, when it suits my purpose; an accusation which I leave your readers to judge whether he ought not to feel ashamed of, when they peruse the following paragraph, which is the only one in my paper alluding, in the slightest degree, to the sentiments of the Cornish miners; a set of people, allow me to remark, much beyond the class in which Dr. Grierson seems desirous to include them:—

"The only other rock of any importance in Cornwall is granite, termed *grauen* by the common people—a name also given to clay porphyry, a substance found pretty frequently in large veins, (Nos. 16, to 19, 28, 48.) The shades of distinction chronicled by the mineralogist, cannot be expected to attract the attention of the miner, who knows but two rocks, grauen and killas, throughout the stannaries. It has been thought that a distinct rock was understood by the term *elvan*; but this is a mistake; elvan may sometimes be greenstone, but in general is killas or granite, and is so termed by the miner when he finds the rock harder to work in one place than in another."

Where or when Professor Jameson discovered the second formation of granite, to which Dr. G. refers that of Cornwall, we have yet to learn. It is possible that such a discovery has led to some of those alterations which that gentleman has found it necessary to engraft upon the system of his master; as we find the second granite, when Mr. Jameson's last book was published, was to be found "only in veins," p. 106. And Dr. Grierson will remember, that I purposely confined myself to the system of the master, not that of the pupils.

I now leave you and your readers to judge whether it was necessary for Dr. G. to have put himself to so much *pain* in recording my misstatements. Next time, however, I would recommend a little more consideration; he may be assured I never will intentionally misstate any fact, and I am not very likely to volunteer my opinion on a subject I do not comprehend.

Allow me again to thank you for the confirmation you have afforded my observations, with regard to the granite and killas of Cornwall.

You have acknowledged that you found granite veins proceeding from the little hill of St. Michael's Mount into the clay slate which rests upon that granite; you found in these veins fragments of the clay slate. You allow that they are true veins; and you yet venture to consider them as the offspring of deposition. You talk of beds which occur at the west side of the Mount,* and you call it in consequence transition granite. But why do you, towards the end of your paper, call the granite of Cornwall primitive granite? Did you find any distinction between that of the Mount and that of Dartmoor? and does not tin occur in veins in both?† And with respect to killas, you declare that substance to be the same with transition slate; and as Mr. Jameson recognises no transition slate, except grauwacke slate or flinty slate, I consider you have made out my case as completely as if you had gone to Cornwall for the purpose. Grauwacke, I have already stated, I found in the most unquestionable shape in different parts of Cornwall; at Grampound you found it alternating with grauwacke-slate; and, lately, I was presented by Mr. Buckland, of Oxford, with a specimen from the vicinity of Probus, as highly characterised as any grauwacke from the neighbourhood of Moffat, where I was first taught by Professor Jameson what that rock was.

I shall now finish this long letter, by informing you, with regard to the age of tin and wolfram, that I have great reason to suspect they are found in no other granite but that of the oldest formation.‡

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* I did examine all sides of the Mount; and I must confess, that I found no appearance whatever that conveyed to me the smallest notion of a *bed* of granite.—[The examination, in that case, must have been but carelessly made; for the granite beds are very conspicuous at low water, and may be easily traced a considerable way.—T.]

† I did not particularly examine the granite of Dartmoor; but there is a striking difference between the granite of St. Michael's Mount and that of the peninsula of Penzance; but had they been perfectly similar it would not have altered my opinion. The terms *primitive* and *transition* allude only to *position*, and this can be determined only by examination. When I find granite lying over *transition slate*, I call it *transition granite*. If I find its characters not to agree with those of Werner's transition granite, I conclude that Werner's induction was imperfect, and that his opinions on the subject are incorrect. At the same time it is by no means doing justice to Werner to compare the opinions which he entertained 13 years ago with our present knowledge. This is what Mr. Allan does. He cannot know Werner's present opinions till he publish his system. I have been told that this is his intention.—T.

‡ This opinion, for any thing that I know to the contrary, may be correct. The granite and the slate in which the tin veins of Cornwall occur I called *primitive* in my paper on the subject, because I saw no proof of their being transition. But if tin veins were to be found in St. Michael's Mount, I should consider this as a sufficient proof that they occur also in transition formations.—T.

This I state on the authority of Mr. Giesecké, a gentleman who has explored more of Europe, with a view to mineralogy, than perhaps any other individual. I am, Sir,

Your obedient humble servant,

Charlotte-square, Edinburgh,
Nov. 25, 1813.

Thomas Allan.

P.S. I had almost forgot the Plymouth Periwinkle, with which you have been so nearly deceived. Give me leave to assure you, that although you were not so fortunate as to find shells imbedded in the limestone of that place, my faith in the accuracy of Mr. Playfair's observation is not in the slightest degree shaken. But whether you found organic remains there or not, is of little importance; when we find shells in the slate itself, it is of no consequence whether they occur in the limestone that accompanies it or not.*

Besides the situations in which I found shells in grauwacke and grauwacke slate, or transition slate if you please, as the *period* is, in point of fact, all we contend about, in your Number for July, I perceive the Rev. Mr. Conybeare met with the same appearances at Clovelly, on the north coast of Devon; and if that be not sufficient, I beg leave to refer you to the magnificent specimen of slate, from Tentagel, in Cornwall, presented to the Geological Society, covered with the impressions of shells—it speaks for itself.

Article VI.

Outlines of the Mineralogy of the Ochil Hills. By Charles Mackenzie, Esq. F.L.S. F.W.S. M.G.S.†

(With a Map.)

If the true ends of science be promoted rather by careful observation than by vague hypothesis, the geognosy of Werner has peculiar claims to admiration. Without the lofty pretensions which constitute the chief distinction of some speculations, it has established general principles, which facilitate the labours of the student, and prompt to continued exertion. A system which developes the great laws of nature, and is substantially improved by the examination of her works, is of all others the best calculated to promote every science; and accordingly we find that mineralogy has made the most rapid advances wherever this has been fairly adopted. Formerly mineralogical inquiries produced nothing more than a mere catalogue of localities; but now many relations of individuals have been distinctly determined; others are daily ascertained, and the most doubtful will probably be accurately known at a period not very remote. The correctness of these observations is shown by the history of mineralogy within a very few years, during which there has been an immense accumulation of geognostic facts collected from portions of the whole known world. In Britain alone has the comparative progress of the science been unequal to the apparent ardour with which it has been pursued, and unworthy of the example afforded by our indefatigable and justly distinguished President. With a view of contributing as far as it is in my power to remedy this deficiency, I have examined the interesting district of the Ochil Hills; and I now beg leave to lay the general results of that examination before the Society. In many instances they will be found imperfect and unsatisfactory. It would have been gratifying had it been possible for me to have made them more complete; but as circumstances render that wish unavailing, I trust that others, whose opportunities may be more favourable, will be prompted to retrace my steps, to correct my errors, and to add new facts to those I shall detail.

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* The distribution of petrifactions seems to follow a regular law, which it should be our object to trace. Petrifactions occur in transition limestone, but hitherto, as far as I know, no shells have been found in that rock, nor any thing else except madrepores and orthoceratites. It is, therefore, an object of considerable consequence to examine carefully the Plymouth limestone, because if Mr. Playfair's observation be verified it will establish a new fact of considerable importance. Shells have been found in transition slate; for I saw a *venus* which Dr. Leach took out of a rock of that kind at Plymouth. The point in discussion would not alter our opinion respecting the rocks in question; but it would add to our knowledge of the distribution of animal petrifactions.—T.

† Read before the Wernerian Society of Edinburgh Nov. 14, 1812.

General Description.

Modern geographers consider the Ochil Hills as the southern boundary of the Grampians; and, in that point of view, the eastern portion may be traced to the Seedlay Hills; while the western extremity should be regarded as a *mountain-arm* stretching into the extensive valley which reaches from the verge of the Grampians, properly so called, to the shores of the Forth: but as the present object is to give a sketch of their particular structure, without entering more into their general relations than distinctness requires, it will be convenient to view them as a *small mountain-group*, which rises above the sea-port of Parton Craigs, on the right bank of the Tay, and, after having skirted the northern parts of Fifeshire, traversed Perthshire, bounded Kinrosshire and Clackmananshire, through a course of more than fifty miles, terminates on the river Allan, near Dunblane, in Perthshire.

This group consists of a high chain, the loftiest point of which, at its first rise,* does not exceed 3 or 400 feet; but more lofty † summits occur to the westward, until Bencleugh[*] and Dalmyatt rear their heads at an elevation of more than 2000 feet above the level of the sea. Several smaller chains may be traced in a course nearly parallel to that of the most elevated, particularly to the south, where they may be distinctly seen gradually diminishing until they are lost in the adjacent valleys. In a few instances, as at the south-eastern extremity, they diverge from the general direction, forming small mountain-arms, which bound some lateral valleys of great fertility and beauty.

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* Craig Law, above the village of Parton Craigs.

† The following are the most remarkable, going from east to west. Norman's Law, Glenduchy Hills, Clachert Craig, the hills of Abernethy, Castle Law, Sea

The individuals of which this group is composed are generally long round-backed hills, very richly covered with verdure, having occasionally conical and rarely tabular summits. Those of the first description are most numerous between Parton Craigs and Abernethy, and those of the latter between Dunning and the Yetts of Muckhart; and it is worthy of remark, that the former are more completely covered up than any of the other hills.

The *acclivities* look to the north, and are generally rapid, though there are some remarkable exceptions to this observation. The *declivities* are very gentle, except at Bencleugh, where they are in many places nearly precipitous. A very large proportion of the Ochil Hills are cultivated to the very summits, and nearly the whole of the remainder are excellent pasture. The natural consequence of this is, that there are few openings, except in an accidental quarry, or some rare natural exposure,[†] circumstances which embarrass the mineralogist in no common degree.

The *dip* of the strata, with very few exceptions, is to the south-east, corresponding with the declivities; and the *direction* from

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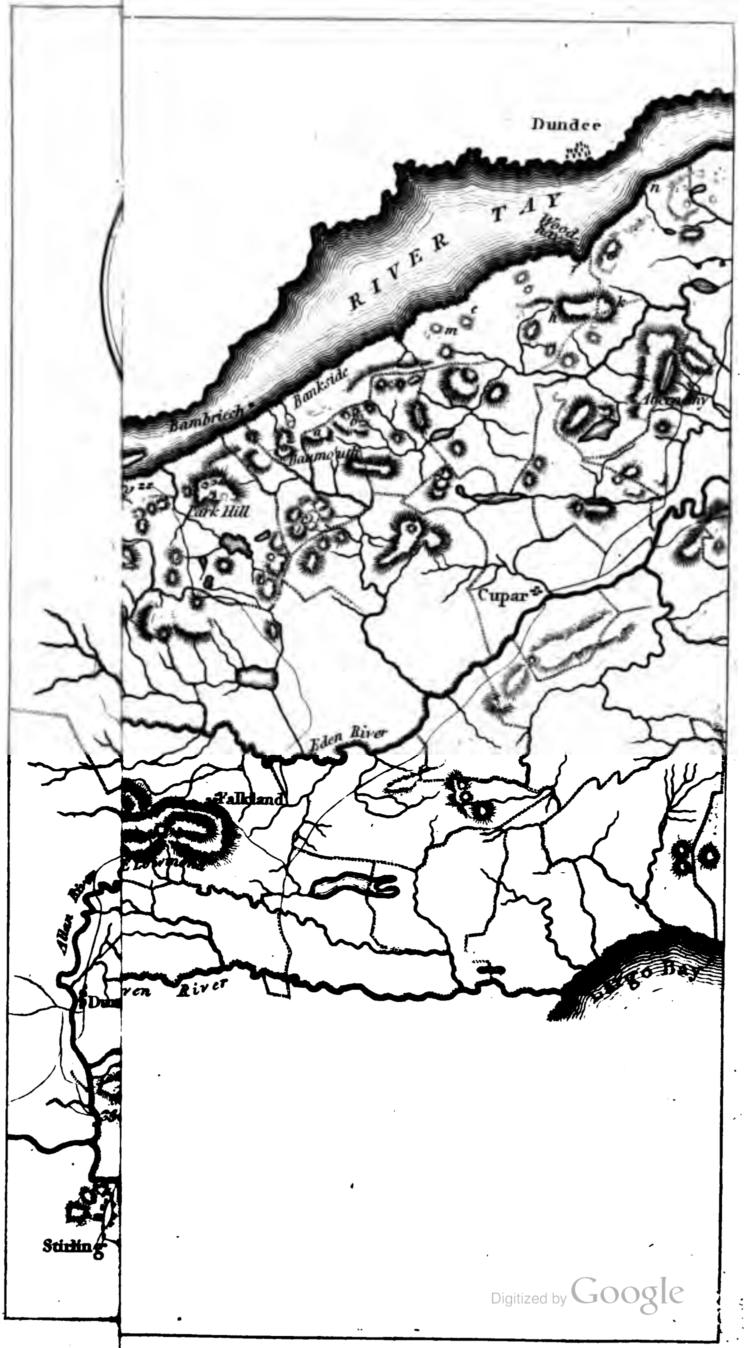
Male, King's Seat, Bencleugh, Dalmyatt; besides many others which may be traced in the map.

⁂ It being impossible to engrave all the names on this map, the following are those referred to by the letters and numbers:—

a Red Hall Hill,
b Norman's Law.
c Dunbog Hill.
d Lindores Loch,
e Scar Hill.
f Wood Lave.
g Wormit Hill.
h Galla Hill.
k Newton Hill.
m Balmerino.
n Newport.
o Lucklow Hill.
p Kilmenie.
q Tarlundie.
r Achtermuchty Hills.
s Binn.
t Beuly.
u Kilnochy.
w Little Fildie.
x Belsvuie.
y Clockrel Stane.
z Muckle Fildie,
1 Fargo.
2 Letham.
3 Old Fargo.
4 Conland.
5 Arngask.
6 Damhead.
7 Birnie Hill.
8 Hill Town.
9 Redford Nook.
10 Blair.
11 White Hill.
12 Abbots Dougal.
13 Spring Hill.
14 Thornton.
15 Moorhead.
16 Yairnit Side.
17 Rintoul.
18 Yetts of Muckhart.
19 Black Hill.
20 White Hill.
21 Glenhead.
22 Glenbee.
23 Carhill.
24 Castle Campbell.
26 King's Seat.
27 Bencleugh.
28 Silver Mines.
29 Alva Hill.
30 Alva.
31 Myretown.
32 Dalmyatt.
33 Blair.
34 Logie.
35 Bridge of Allan.

[*] Above the village of Westertown, &c.

[†] Such is exhibited in the magnificent range of columns which may be traced nearly from Craig-in-Crune to Clachert Craig.


Dundee
RIVER TAY
Wood Haven
Bankside
Bambriech
Balmouth
Park Hill
Abernethy
Cupar
Eden River
Falkland
Lowmonds
Allan River
Dun
ven River
Largo Bay
Stirling

north-east to south-west, corresponding with the direction of the whole group, which runs about half of its course from east to west, and then, changing its direction, runs from north-east to south-west.

Springs are very numerous throughout the whole of the Ochil Hills; in some instances they form pools, in others bogs, and in many they unite and give rise to several beautiful small streams, that meander through the neighbouring districts. The Devon, the Allan, and the May, are the most remarkable of these streamlets; and they pour their waters into the Forth, the Tay, and the Erne.

The *valleys* are also numerous, separating the several chains and the individual heights from each other. In general they are narrow, not exceeding 30 or 40 yards in width; their length depends on that of the chain or mountain which they bound. These valleys are most numerous at the western extremity of the Ochils, where they are also most extensive. They imperceptibly diminish both in size and importance to the eastward. It is, however, in the latter portion that the romantic valley occurs, in which the Lake of Lindores is contained. In this small spot nature has crowded together all that can delight the eye and elevate the imagination.

The Ochil Hills are bounded to the north and north-west by the Frith of Tay, Strath-Erne, and Strath-Allan; to the south-west by the vale of Forth; to the south by the vales of Devon, of Kinross, and of Eden; and to the east by the left bank of the Eden, where it is lost in the right bank of the Tay.

The prevailing rock throughout the whole of the northern boundaries is a dark brick-red sandstone, which extends as far as Callendar to the west, and to Stonehaven * to the east, and is in all probability the *old red sandstone*. The independent coal formation, according to Mr. Bald,† forms the coal-field of Clackmananshire and Stirlingshire; and the red sandstone, occasionally assuming the characters of conglomerate,‡ again occupies nearly the whole of the valleys of Kinross and Eden; while grey sandstone, slate-clay, bituminous shale, pitch-coal, and clay-ironstone, form the right bank of the Eden, to the south of St. Andrew's. On the left bank, which is more immediately contiguous to the Ochils, beds of sand, clay, and marl, have been observed.

The rocks composing the Ochil Hills occur in nearly the following order:—

1. Red sandstone.
2. Amygdaloid.
3. Grey sandstone.
4. Limestone.
5. Slate-clay.
6. Claystone.
7. Tuff.
8. Basaltic clinkstone.
9. Greenstone.
10. Claystone porphyry.
11. Felspar porphyry.
12. Compact felspar.

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* See Colonel Imrie on the Conglomerate, &c. vol. i. Wernerian Memoirs.

† See Mr. Bald on the coal-field of Clackmananshire, vol. i. Wernerian Memoirs.

‡ This is distinctly the case immediately to the north of Cupar, in a small opening made by the rivulet which empties itself into the Eden below the town.

1. *Red Sandstone.*—This rock occurs for the first time, in travelling from east to west, on the shore below Birkhill. It is also found in a quarry between Bambriech Castle and Newburgh; in the small hills between Pitcaithly* and Dunning; and to the westward of the latter place. Its colour is a dark brick-red, brownish red, and reddish grey. It is coarse-grained, occasionally it becomes conglomerated, as to the south of Dunning, where it rests on the reddish-grey sandstone, and contains considerable masses of quartz, fine-grained sandstone, and scales of silver-white mica. This sandstone is occasionally highly crystalline, bearing some resemblance to iron-flint. When the mica predominates, it assumes a slaty character, and decomposes into tables.

It occurs distinctly stratified, dipping to the south-east, with an apparent direction from north-east to south-west. I have not seen it in connection with any other rock, except below the Rumbling Bridge, in the course of the Devon, where it alternates with a tuff; and at the foot of the King's Seat, near to the house of Harvieston,† where it rests immediately above a seam of slaty pitch-coal, six feet two inches thick. From the resemblance of its characters to those of another sandstone to be hereafter noticed, it is highly probable that they will be found to belong to the same formation; but as they have not been traced in distinct connection, it may be well to keep them separate at present. At the base of Alva Hill it seems to lie below greenstone.

In a small valley which traverses the Ochils, between Wormit Bay and a lateral valley that divides Newton Hill from Sanford Hill, there are several small hillocks of an ironshot sand, which contains masses of this sandstone. It is probable that they have been derived from the decomposition of the red sandstone just described.

Although it has not been accurately determined, it is highly probable, that this red sandstone, from the number of points at which it occurs, and the coincidence between its characters and those of the *old* red sandstone which occupies the adjacent valleys, that they will be hereafter found to be intimately connected. At present I shall hesitate to fix the place of this rock in the system, and content myself with observing, that it seems to be the lowest of the series composing the Ochil Hills.

2. *Amygdaloid.*—On the shore between Parton Craigs and Balmerino (a district of nearly nine miles), a coarse amygdaloid gradually passes into a finer variety of the same rock. The former of these consists chiefly of portions of the latter, binding together various substances. I could not discover the thickness of any of the beds; but I apprehend, from having seen a different rock at a small height above it, that it is inconsiderable. The basis of this rock is a greyish-green claystone, occasionally very much ironshot.

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* This is a small village in Perthshire, celebrated for a mineral spring, to which considerable efficacy has been ascribed.

† The seat of my friend Crauford Tait, Esq.

The numerous cavities contained in it are lined with white amethyst, flesh-red calcareous spar, white felspar, chalcedony, red flint, and common quartz. The chalcedony appears to have been first deposited, and the quartz to have been the last.

The amygdaloid sometimes becomes porphyritic, containing crystals of felspar.

It is difficult to assign to this rock its correct geognostic position. At Parton Craigs it is below claystone porphyry. Near Newport it alternates with basaltic clinkstone. At the western extremity of Wormit Bay it is below the claystone, through which it seems to be connected with the tuff on the one hand, and with the claystone porphyry on the other. On the water of Fargs, which runs from Damhead* to Abernethy, it occurs resting on a variety of greenstone, which is connected with the clinkstone. In this last situation it possesses a great variety of characters, being extremely coarse at one point, where a bed of clay porphyry rests upon it. More to the southward the vesicles in the amygdaloid become very distinct, and contain a flesh-red variety of felspar.

This rock is traversed by veins of calcareous spar, which exhibit a sea-green colour when fresh broken.

3. *Grey Sandstone.*—Above the amygdaloid† beds of a yellowish grey sandstone alternate with tuff and claystone, which appear to be intimately connected with some varieties of the amygdaloidal rock. It contains a considerable quantity of scales of silver-white mica, and decomposes into slaty masses. It seems to pass almost imperceptibly into the varieties of claystone to be hereafter described. One variety of this rock has a very remarkable granitic appearance, and does in fact contain all the constituents essential to true granite. At the same time, however, it retains the distinctive characters of sandstone.

The relations of this rock to the older rocks is not clearly made out, except at Wormit Bay, where it seems to rest on amygdaloid. It occurs frequently between Wormit Bay and the village of Dunning, in Perthshire. It also occurs in the course of the Devon, where it may be seen alternating with the red sandstone, to which in all probability it belongs.

Its general dip is to the south-east, and its direction from north-east to south-west.

4. *Limestone.*—In a quarry at the base of Park Hill, not far from Bambriech Castle,‡ a saddle-shaped mass of yellowish grey limestone rests between beds of slate-clay and grey sandstone; and above the newest sandstone there is a bed of greenstone, the upper

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* This place is about half way between Kinross and Abernethy.

† At the western extremity of Wormit Bay.

‡ Bambriech Castle stands on a promontory of red sandstone, which runs a short way into the Tay, from its southern bank, about three miles to the eastward of the small sea-port of Newburgh. Immediately to the south of the castle the Ochils rise very rapidly, and receive different names. That of Park Hill is appropriated to the hill half way between Newburgh and Bambriech.

part of which is much decomposed. This limestone varies from an earthy to a highly crystalline structure, resembling if not passing into calcareous spar.

This is the only instance throughout the whole of the Ochils that I have seen limestone, although I have been assured that other portions of it have been observed at a considerable elevation above the village of Abernethy. On examining the situations to which I was referred I could discover no traces of limestone, from which I have been induced to suspect that the popular belief is unfounded.

5. *Slate-clay.*—Thin seams of a bluish-grey slate-clay, possessing the common characters, occur both above and below the limestone, separating it from the grey sandstone.

6. *Claystone.*—This is a very abundant rock, and some very beautiful varieties of it occur in the course of the Ochil Hills. At Lucklaw* it appears to pass into basaltic clinkstone; below Birkhill † it alternates with sandstone, tuff, and felspar, passing on the one hand into the grey sandstone, and on the other, through all the varieties of tuff and clinkstone, to the perfectly compact felspar. It is also found between the Yetts of Muckhart and Alva, below the clinkstone, with which it probably alternates.

It is fine grained, having a large flat conchoidal fracture, and an uneven cross fracture. It occasionally contains scales of silver-white mica, particularly where it alternates with the sandstone and tuff, as it does below Birkhill. The colour is various, even in the alternating strata; the most common, however, is between pearl grey and Isabella yellow. It sometimes, though rarely, is amygdaloidal: the cavities of such varieties are filled with green earth and white calcareous spar: crystallized felspar is dispersed throughout the mass.

The claystone alternates with the grey sandstone and the tuff, between which it is most probably the connecting link; for it passes almost imperceptibly at its extremities into each of them. It is doubtful whether or not the claystone be of older formation than the limestone, as I have seen both in similar relations to the sandstone. The present location, therefore, of these rocks, in so far as they regard each other, must, until more extended observations shall have been made, be considered as entirely arbitrary.

7. *Tuff.*—A very remarkable tufacious rock occurs above the claystone at the base of Birkhill, and at the western extremity of Wormit Bay. In both of these situations it alternates with the sandstone and claystone. In no other part of the Ochils have I observed a similar arrangement.

This tuff is coarse, inclosing portions of felspar, which is sometimes lost in the prevailing mass. The chief colours are flesh-red and Isabella yellow. It appears to be one of the newest members of the sandstone series, and there is a gradual passage from it to the claystone. It is distinctly stratified, having the general dip, direction, and inclination, of the whole mountain-group.

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* This is a small hill between Cupar and Parton Craigs.

‡ An eminence about five miles to the westward of Woodhaven.

8. *Basaltic Clinkstone.*—From the first rise to the final termination of the Ochil Hills, clinkstone is the prevailing rock. It occurs at Parton Craigs, resting on, and in one instance alternating with, the amygdaloid; from Craig-in-Crune (half way between Woodhaven and Newburgh), it forms the summits of the hills, occasionally exhibiting columns of more than 100 feet in height, which rise precipitously from the low lands on the south bank of the Tay, and produce a noble and imposing effect.* At the more western portions of this district the clinkstone is connected with greenstone, felspar porphyry, and compact felspar. At Westerton, immediately above the junction with the coal-field of Clackmananshire, it occurs distinctly stratified, the beds being separated from each other by thin seams of leek-green steatite, which contains iron pyrites in considerable abundance. It occasionally assumes the characters of basalt; at other times it is more decidedly clinkstone; but it most generally possesses characters intermediate between those of basalt and clinkstone; from which circumstance I have been induced to adopt the name of basaltic clinkstone, which applies equally to every variety.

Its colours are blackish grey, blackish brown, and sometimes it is much ironshot, particularly at the summits of the lower hills. Its fracture is slaty and rough, and in general it emits the clinking sound to which the species owes its name. Beautiful specimens of an amygdaloidal variety occur between Abernethy † and Kinross.

The dip and direction of the stratified portions of this clinkstone correspond with the general dip and direction.

9. *Greenstone.*—Throughout the district which extends from Parton Craigs to Newburgh the clinkstone frequently passes into greenstone; and in the immediate vicinity of the latter place it appears distinctly columnar, though its relations to every other rock are wholly undefined.

It is not improbable, from similar greenstone being found in higher portions of the hills between Newburgh and Woodhaven, that it alternates with the clinkstone. Between Dunning ‡ and the Yetts of Muckhart it ocurs frequently above the clinkstone and below felspar porphyry; but is seen in greatest abundance, variety, beauty, and distinctness, between the Yetts of Muckhart and the western extremity of the Ochils, particularly in an exposure made by a streamlet which divides the King's Seat from Craiginnan,* and in a section above the village of Westertoun. In this district it forms separate hills, or their caps; and in the central parts of the group it alternates distinctly stratified with the basaltic clinkstone, which it connects with the felspar rock through the felspar porphyry. The section above Westertoun may be considered a beautiful epitome of these alternations, and it receives an additional interest from its exhibiting a fine view of the junctions of the coal-field with the newer rocks. The beds of greenstone have a dip and direction at right angles, to the dip and direction of the clinkstone, from which it is separated by thin seams of decomposing steatite. The beds of the greenstone itself are also separated by thin seams of this steatite, which contains considerable quantity of iron pyrites. The gradation † from the rock in which the hornblende predominates to that in which a beautiful flesh-red felspar prevails, is marked in a series of six alternating portions of greenstone and clinkstone, which commence at the above-named section, and may be traced in the face of Bencleugh beyond Alva.

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* This is remarkably the case in the hills between Craig-in-Crune and Norman's Law. The columns there have a diameter of from five to seven feet. They are pentagonal, as it usually happens.

† The hills in this district are very picturesque, and have, I believe, appropriate names, though I could not learn them, as every shepherd furnished one of his own.

‡ The observation made on the hills between Abernethy and Kinross applies to those of the above portion of the Ochil Hills. The confusion arising from a perpetual repetition of names has induced me, in many instances, to omit them altogether.

It is worth recording, that a bed of greenstone occurs in a coarse conglomerated rock in the hill of Balcanquhal.‡ It is of small extent, and may be seen in all sides except at its base. There can therefore be no doubt of its relations to the rock in which it is imbedded, from the characters of which it may be fairly presumed that it does not owe its existence to volcanic agency.§

The characters of the greenstone are those which commonly occur, except in the higher alternating beds, where they assume those of the rock which Mr. Jameson calls sienitic greenstone. It is occasionally porphyrytic, containing fine and indistinct crystals of rutilite.

10. *Claystone Porphyry.*—On the south side of the Abernethy Hills a bed of flesh-red claystone porphyry, with crystals of glassy felspar, is above the clinkstone, and some varieties of greenstone, but its relations are wholly undefined.

11. *Felspar Porphyry*—Forms the caps of the highest hills which lie between Dunning and Dunblane. It is a compact flesh-red felspar, containing crystals of white calcareous ‖ spar. In the course of the streamlet which runs past Castle Campbell it alter-

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* Craiginnan is the hill which rises immediately behind Dollar, and is connected by a series of conical hills with the romantic and precipitous Craig Rossie, which rises to the westward of the village of Dunning, in Perthshire.

† It is a curious fact that all the red varieties of rock that I have observed in the Ochils occur at the highest points. It is difficult to form even a conjecture as to the cause of this.

‡ About three miles from Kinross, to the north of the road between that place and Cupar.

§ If this bed of greenstone were spouted up from the centre of the earth the intensely hot fluid mass must have acted on the bed through which it flowed. But of this action there is no evidence.

‖ This appears to be intimately connected with the sienitic greenstone. Some beautiful masses of this rock are to be seen higher than the greenstone at Craig Rossie, near to Dunning.

nates with greenstone. It occurs in decomposed fragments on the summits of Craiginnan, King's Seat, Bencleugh, and Dalmyatt. These decomposed fragments have a vesicular appearance, from a very obvious cause (the rapid decomposition of the included crystals). This appearance would no doubt be ascribed to volcanic agency, by those whose zeal for hypothesis outstrips their love of accuracy.

Only the lower portions of this rock contain crystals of hornblende, which give way at the higher points to those of calcareous spar.

12. *Compact Felspar.*—Very beautiful brick-red and flesh-red compact felspar, possessing all the usual characters, forms the caps of some of the smaller hills of the southern chain of the Ochils, which extends from the neighbourhood of Dollar to the banks of the Eden. Near Cupar it occurs so abundantly as to be the sole material for repairing the roads. It appears to be the newest member of the series, and to correspond both in its individual characters and in its geognostic relations with the felspar of the Pentland Hills, where it was first noticed forming distinct masses by Professor Jameson. At the summit of Lucklaw this felspar passes into hornstone. A solitary bed of it is to be found in the alternating series of sandstone, claystone, and tuff, in Wormit Bay.

VEINS.

Having thus briefly noticed the prevailing rocks, I shall now mention the veins traversing these rocks, in the order of the strata in which they occur. Contemporaneous veins are not uncommon in the clinkstone and greenstone; but true veins are more rare. The following is the order of the latter:—

1. Calcareous spar. 2. Steatite.
3. Heavy spar.
4. Iron. 5. Cobalt. 6. Silver.
7. Copper. 8. Lead.

Calcareous Spar.—Highly crystallized varieties of the calcareous spar traverse the clinkstone near Woodhaven, and the claystone near to the Rumbling Bridge. It has a greenish tint, and all the usual characters. The dip and direction not easily determined. The thickness from half an inch to two inches.

Steatite.—Veins of this steatite, varying from one to two inches, divide the strata of the clinkstone and of the greenstone at the section above Westertoun and Alva. They occasionally contain iron pyrites crystallized, in cubes, in considerable quantities.

Heavy Spar.—Straight lamellar heavy spar is the veinstone of the mine behind Castle Campbell, of those of Alva Hill, and Airthry Hill, in all of which it traverses the newer varieties of the clinkstone which approach to felspar through the greenstone. They are four or six feet wide, with their outgoings to the south. Dip near 45° to the north-east; but from the falling in of the roofs and other accidents it was impossible for me to ascertain any particulars respecting them. It is in these veins of heavy spar that the cobalt, silver, copper, and lead, have been deposited. Of the first two of these I could discover no trace, though there is no doubt that both have been obtained in considerable quantity. The fullest account I have been able to meet with of the silver mines is that contained in Sir John Sinclair's Statistical Account of Scotland, under the head Alva. Of the cobalt I have seen no published account. It appears that both these metals were found in the Alva Hill.

Copper and lead are still to be found both in the mine behind Castle Campbell and in the mines of Alva. But the specimens (which alone I could procure) at the entrance, are so much acted upon by the weather, that I cannot venture to attempt any description of the varieties of ore that occur. The first named of these mines has been twice opened since the year 1760, but has been abandoned an both occasions. But it is not determined whether or not the want of success has arisen from mismanagement or from the unproductiveness of the mine. From their present state, I found it impossible to make any accurate observations on them.

Such are the most important facts that I have noticed in the examination I have been able to give to the Ochils. Although the information I have collected be imperfect, I trust that the Society will receive it as a pledge of my readiness to contribute all that I can to the science of mineralogy. It may be expected that I should assign the geognostic place of the Ochils. With the limited observations that I have made, I could do no more than throw out conjectures, which would not conduce to the legitimate ends of science. Until more extended examinations shall have been made, I must beg leave to confine myself to a simple narrative of facts.

ARTICLE VII.

On the Antilunar Tide. By John Campbell, of Carbrook, Esq. F.R.S.E.

It may perhaps appear not a little presumptuous in one whose professional avocations do not afford the leisure, even had he the requisite talents for abstruse speculation, to call in question any received doctrine of the Newtonian philosophy. I feel the force of the personal objection, and yet venture to submit some observations on the theory of the inferior or antilunar tide.*

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* Although the Newtonian theory of both tides be that which, in its full extent, is generally taught in the school of philosophy, the obvious difficulties attending the solution it gives of the cause of the antilunar tide have not failed to stagger many an acute mind. Mr. Ferguson was so much afraid of encountering them, that, rejecting the principle altogether, he ascribed the two tides, notwith-

To prevent misconception it will be proper in the outset to give a distinct view of this part of the Newtonian system; and for this purpose I cannot do better than adopt the words of one of the most profound, as well as perspicuous, of Newton's disciples: I mean Mr. M'Laurin, whose Treatise on the Tides has been held to contain such a complete exposition of the principles on which their existence depend, that succeeding philosophers have directed their labours rather to prove the agreement of the irregularities of the tides with these principles than to investigate the truth of the principles themselves.

After describing the spherical figure which the earth, if entirely fluid and quiescent, would assume; and showing that the introduction of any other force, acting equally on all the particles, and in parallel lines, would not alter this figure, he thus proceeds:—

"But the actions of the moon on different parts of the earth are unequal, those parts by the general law being most attracted which are nearest the moon, and those being least attracted which are farthest from the moon, while the parts that are at a middle distance are attracted by a mean degree of force. Nor are all the parts acted on by parallel lines, but in lines directed towards the centre of the moon; and on these accounts the spherical figure of the earth must suffer some change from the moon's action.

"Suppose the earth to fall towards the moon, and let us abstract from the mutual gravitation of its parts towards each other, as also from their cohesion, and it will easily appear that the parts nearest the moon would fall with the swiftest motion, being most attracted, and that they would leave the centre or greater bulk of the earth behind them in their fall; while the more remote parts would fall with the slowest motion, being less attracted than the rest, and be left a little behind the bulk of the earth, so as to be found at a greater distance from the centre of the earth than at the beginning of the motion: from which it is manifest that the earth would soon lose its spherical figure, and form itself into an oblong spheroid, whose longest diameter would point at the centre of the moon.

"Let us now allow the parts of the earth to gravitate towards its

standing their evident coincidence, to different and unconnected causes; causes which, unfortunately for his theory, have no existence in nature. M. Laplace, while, without remark, he adopts the principle, admits that in several respects the Newtonian theory is erroneous; and there is some reason to hold, as will afterwards appear, in considering their opinions on the causes which produce elliptic orbits, that both Laplace and M'Laurin, the expositor of Newton, ought to be ranged in opposition to the principles they have inadvertently admitted. It may not be without some weight, also, to mention, that the late able Professor of Natural Philosophy in the Edinburgh University, Mr. John Robison, when pressed upon the objection to the cause assigned by Newton for the swelling of the antilunar tide, acknowledged that the theory was applicable only to the case of a fluid globe, and not to the shallow waters on the surface of the earth. I am satisfied that the cause assigned is equally erroneous, whether applied to a fluid globe or shallow waters; but it is sufficient to state the above acknowledgment, contained in a letter to myself, to show that Professor Robison was satisfied that the Newtonian theory was inapplicable to the antilunar tide.

centre; and as this gravitation far exceeds the difference of her (the moon's) actions on the different parts of the earth, the effect that will result from these inequalities of the actions of the moon will be only a small diminution of the gravity of those parts of the earth, which it endeavoured, on our former supposition, to separate from its centre; and a smaller addition to the gravity of those parts which it endeavoured to bring nearer to its centre, that is, those parts of the earth which are nearest to the moon, and those which are farthest from her, will have their gravity towards the earth somewhat abated; whereas the lateral parts will have their gravity somewhat increased; so that if the earth be supposed fluid, the columns from the centre, to the nearest and to the farthest parts, must rise till, by their greater height, they be able to balance the other columns, whose gravity is either not so much diminished or is increased by the inequalities of the action of the moon; and thus the figure of the fluid earth must be still an oblong spheroid.

" We have hitherto supposed the earth to fall towards the moon by its gravity. Let us now consider the earth as projected in any direction, so as to move round the centre of gravity of the earth and moon. It is manifest that the gravity of each particle towards the moon will endeavour to bring it as far from the tangent in any small moment of time as if the earth was allowed to fall freely towards the moon, in the same manner that any projectile at our earth falls from the line of projection, as far as it would fall by its gravity in the perpendicular in the same time. Therefore the parts of the earth nearest to the moon will endeavour to fall farthest from the tangent, and those farthest from the moon will endeavour to fall least from the tangent of all the parts of the earth, and the figure of the earth therefore will be the same as if it fell freely towards the moon; that is, the earth will affect a spheroidal form, having its longest diameter directed towards the moon."*

The theory now explained may be resolved, I apprehend, into this short and simple proposition: that the waters nearest the moon are drawn from the earth; and on the opposite side, the earth is drawn from the waters; in both of which cases they swell into a tide.

The first part of the proposition has never been contraverted; but that the earth is drawn from the waters, in order to form the antilunar tide, is a point by no means so easily conceded. To me it appears to be contrary to known facts, to the general principles of gravity, and to the more matured opinions of Mr. M'Laurin himself.

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* The theory, as condensed by M. Laplace, is as follows:—" Une molécule de la mer, placée au dessous du soleil, en est plus attirée que le centre de la terre; elle tend, ainsi, a se séparer de sa surface; mais elle y est retenue par sa pesanteur, que cette tendance diminue. Un demi jour aprés, cette molecule ce trouve en opposition avec le soleil, qui l'attire alors plus faiblement que le centre de la terre; la surface du globe terrestre tend donc a s'en separer; mais la pesanteur de la molecule l'y retient attachée; cette force est donc encore diminuée, par l'attraction solaire, et il est facile de s'assurer, que la distance du soleil a la terre, etant fort grande relativement au rayon du globe terrestre, la diminution de la pesanteur dans ces deux cas, est a tres-peu pres la meme."—*Systome du Monde*, p. 274.

The discussion of the question will be much simplified, and the results rendered more satisfactory, by a previous examination of the effects of the general principles which regulate the moon's orbit round the centre common to the moon and the earth, the eccentricity of that orbit being explained on the same principles as the phenomena of the tides, and these effects being so much greater than those produced on the figure of the earth.

For the explanation of these effects upon the moon's orbit, we shall again take Mr. M'Laurin as our guide. "Let us suppose," says he, "that the projectile motions of the earth and moon were destroyed, and that they were allowed to fall freely towards the sun. If the moon was in conjunction with the sun, or in that part of her orbit which is nearest to him, the moon would be more attracted than the earth, and fall with greater velocity towards the sun, so that the distance of the moon from the earth would be increased in the fall. If the moon was in opposition, or in the part of her orbit which is farthest from the sun, she would be less attracted than the earth by the sun, and would fall with a less velocity towards the sun than the earth, and the moon would be left behind by the earth, so that the distance of the moon from the earth would be increased in this case also. If the moon was in one of the quarters, then the earth and moon being both attracted towards the centre of the sun, they would both directly descend towards that centre; and by approaching to the same centre, they would necessarily approach at the same time to each other; and their distance from one another would be diminished in this case. Now, wherever the action of the sun would increase their distance, if they were allowed to fall towards the sun, there we may be sure the sun's action, by endeavouring to separate them, diminishes their gravity to each other; wherever the action of the sun would diminish their distance, then the sun's action, by endeavouring to make them approach to one another, increases their gravity to each other; that is, in the conjunction and opposition their gravity towards each other is diminished by the action of the sun, but in the two quarters it is increased by the action of the sun. In considering, therefore, the effects of the sun's action on the motions of the earth and moon, with respect to each other, we need only attend to the excess of its action on the earth in their conjunction, and we must consider this excess as drawing the moon *from* the earth *towards* the sun in that place. In the opposition we need only consider the excess of the action of the sun on the earth above its action on the moon, and we must consider this excess as drawing the *moon from* the *earth* in this place in a direction *opposite* to the *former*; that is, to the place opposite to where the sun is, because we consider the earth as quiescent, and refer the motion and all its irregularities to the moon."[*]

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[*] According to Laplace—" Dans ses conjonctions avec le soleil, la lune en est

It is obvious that this is no other than the preceding theory of the tides, the moon's orbit being the oblong spheroid, which wants only a rotatory motion round its axis to exhibit the phenomena of the ebb and flow.

That the eccentricity of the moon's orbit is materially affected by gravitation is an admitted fact—the point at issue is the mode of application. And it may be observed in the outset, that the exposition given in the preceding passage is contrary to all analogy. All the planets, primary as well as secondary, move in nearly similar, that is to say, elliptical, orbits: it would be natural, therefore, to expect that one law should regulate them all; yet the cause of their approaching and receding, of their increasing and diminishing curvature, is explained by an application of the principles of gravity, differing very widely indeed from that contained in the paragraph just quoted.

The planets are retained in their eccentric orbits by a beautiful combination of these two laws, viz. that gravity or the centrifugal force is governed by nearly the *squares*, while velocities and the centrifugal forces are governed by the *cubes*, of the distances. The centrifugal forces increase or decrease more rapidly than the gravities; while therefore gravity, preponderating in the higher apsis, makes the planet approach to the sun, the centrifugal force prevailing in the lower apsis, enlarges the curve and makes it again recede from him, and by their combined actions the planet continues to revolve from the one apsis to the other. That the eccentricity of the moon's orbit is governed by the general law which regulates the other planets, and not by the difference of the attractions of the sun and the earth, is abundantly obvious; for at the period of opposition the moon is acted upon by the *sum* of the attractions of the sun and the earth, instead of being affected by their difference; and we have the testimony of Mr. M'Laurin himself to this view of the subject. In a paper on the Path of Secondary Planets, written some years after his Theory of the Tides, and when the difficulties of that theory did not trammel his mind, Mr. M'Laurin, after explaining why the moon, whose gravity at the conjunction is greater towards the sun than its gravity towards the earth, does not abandon the earth, proceeds thus: "But it may contribute towards removing this difficulty to observe that if the velocity of the moon at the conjunction was less than that which is requisite to carry a body in a circle there round the sun, supposing this body to be acted on by the same force which acts there on the moon (*i. e.* by the excess of her gravity towards the sun above her gravity towards the earth) then the moon would

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plus pres que la terre, et en eprouve une action plus considerable; la difference des attractions du soleil sur ces deux corps tend donc alors a diminuer la pesanteur de la lune vers la terre. Pareillement dans les oppositions de la lune au soleil, ce satellite, plus éloigné du soleil que la terre, en est faiblement attiré; la difference de l'action du soleil tend donc encore a diminuer la pesanteur de la lune," &c p. 228.

indeed abandon the earth; for in that case the moon, having less velocity than would be necessary to prevent her from descending within that circle, she would approach to the sun and recede from the earth. But though the absolute velocity of the moon at the conjunction be less than the velocity of the earth in the annual orbit, yet her gravity towards the sun is so much diminished by her gravity towards the earth that her absolute velocity is still much superior to that which is requisite to carry a body in a circle there about the sun that is acted on by the remaining force only. Therefore, from the moment of the conjunction, the moon is carried without such a circle, receding continually from the sun, to greater and greater distances, till she arrive at opposition, where, being acted on *by the sum* of those two gravities, and her velocity being now less than what is necessary to carry a body in a circle there about the sun that is acted upon by a force that is equal to that sum, the moon thence begins to approach to the sun again. Thus she recedes from the sun, and approaches to it, by turns; and in every month her path has two apsides, a perihelium at the conjunction, and an aphelium at the opposition, between which she is always carried, in a manner similar to that in which the primary planets revolve between their apsides."[*]

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[*] After stating that the law of gravity in regard to the planets varies reciprocally as the square of the distance, Laplace, with similar views, proceeds thus:—"Son mouvement elliptic ne laisse aucun doute a cet egard. Pour le faire voir, suivons ce mouvement, en faisant partir la planéte, du perihélie, la vitesse est alors a son *maximum*, et sa tendance a s'éloigner du soleil, l'emportant sur sa pesanteur vers cet astre, son rayon vecteur augmente, et forme des angles obtus avec la direction de son mouvement; la pesanteur vers le soleil, decomposée suivant cette direction, diminue donc de plus en plus la vitesse jusqu'a ce que la planéte ait atteint son aphélie. A ce point, le rayon vecteur redevient perpendiculaire a la courbe; la vitesse est a son *minimum* et la tendance a s'éloigner du soleil etant moindre que la pesanteur solaire, la planéte s'en rapproche en decrivant la seconde partie de son ellipse. Dans cette partie sa pesanteur vers le soleil, accroit sa vitesse comme auparavant, elle l'avoit diminuée, la planéte se retrouve au perihélie, avec sa vitesse primitive, et recommence un nouvelle revolution semblable a la precedente."—*Syst.* p. 194. Laplace has not exposed himself to such a direct charge of inconsistency as Mr. M'Laurin; but there occurs, it is apprehended, the same contradictory principles; for it is difficult to anticipate how the moon, which is a satellite of the earth, is to be excluded from the following general observation. Alluding to the satellites of Jupiter, he says, "ils nous offeront par la promptitude de leur revolutions tous les grands changemens que le temps ne developpe qu'avec une extrême lenteur, *dans le systeme planetaire, dont celui des satellites est l'image.*"—*Syst.* p. 243. If the satellites are governed by the same principles as the planets, which is the position here maintained, and which Mr. M'Laurin illustrates in the passage already quoted, then it is to be inferred from the statements of M. Laplace that the elipticity of the moon's orbit, like that of the planets, is owing to the combination of the laws of motion and gravity.

(*To be continued.*)

Article VIII.

Account of a new Explosion of Inflammable Air in the Coal-Mine at Felling, near Gateshead. Communicated by Φίλελξεινός.

Scarcely a year has elapsed since we recorded the loss of 91 persons by an explosion in the colliery called Brandling Main, at Felling, near Gateshead.* At half past one o'clock in the morning of the 24th Dec. this mine again exploded, and killed 23 persons and 12 horses; 21 persons escaped alive, 13 of whom were severely burnt, but are all likely to recover. Only one horse was saved. The persons whose work laid up the south headways, from the upcast shaft, were all destroyed. Those in the boards on the north and east were saved. Some suppose that the inflammable air took fire at the crane lamp, in the south headways, as the persons, and the materials of the mine, on the outside of it, were much shattered; but those on the inside of it had suffered little violence, the men having perished by the choak-damp. This explosion was every way much less severe than the former; but as it happened when the morning shift of men were relieving the night shift, it might have been more destructive in its effects than it has been. A group of the fresh men were waiting to go down; and those who had just descended met the fatal whirlwind of fire in their way to the southern boards, which lie under the village of High Felling. That part of the mine is intersected with several dykes, or fissures, which not unfrequently discharge great quantities of inflammable air, through apertures called blowers, and which make the coals on the floor dance round their orifices, like gravel in a strong spring. Whether this accident is to be attributed to one of these foul discharges, or to the falling of some stopping, which prevented the regular ventilation of the wastes, or to some neglect of standing orders at the rarifying furnace on the upcast shaft, it is perhaps now impossible to discover: but this is certain, that so powerful was the stream of fresh air in all the working parts of the mine, that the candles could with difficulty be kept from being blown out; and the persons employed in it were unanimous in declaring, that they never wrought in a pit so wholesome and pleasant; and that every means were provided, and most judiciously and actively employed to prevent its exploding. Among the sufferers are the overmen, Mr. Wm. Haswell and Mr. Robert Morrow; and their deputies, Mr. Martin Greener and Mr. Robert Stoves. 10 of the bodies were got out on Friday, 6 on Saturday, 2 on Sunday, and 4 on Monday. They were all buried at Heworth. 8 widows, 15 children, and a helpless old woman, who depended on her son, have lost their whole means of support by this calamity; the interests of several others have been partially affected by the loss of sons; but all of them have had great attention and humanity shown them by the owners of the colliery. In the progress of procuring the bodies no part of the mine appeared to have been set on fire: not a cinder or singed prop was seen in any part of it. On Tuesday the ventilation for the renewal of working it was commenced, and regularly carried on till Thursday morning, about eight o'clock, when the workmen were suddenly alarmed by finding the coal on fire in a part of the waste. From this circumstance it was immediately determined to close up the mouths of the shafts.

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* Annals of Philosophy, vol. i. p. 355.

ARTICLE IX.

Astronomical and Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude 51° 32′ 40″ North. Longitude West in Time $6''\frac{82}{100}$.

Emersion Jupiter's 3d Satellite, Jan. 9, 1814	**Mean Time H. W.**	**11^{h}**	**10′**	**53″**
	Ditto at Greenwich	**11**	**1**	**00**

Magnetical Observations.

1813.

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Dec. 18	—^{h}	—′	—°	—′	—″	1^{h}	50′	24°	18′	54″		Discontinued, from the shortness of the days.
Ditto 19	8	55	24	14	29	1	53	24	17	48		
Ditto 20	—	—	—	—	—	1	55	24	18	20		
Ditto 21	—	—	—	—	—	1	50	24	20	32		
Ditto 22	8	55	24	16	45	1	50	24	20	09		
Ditto 23	8	50	24	17	30	1	50	24	20	25		
Ditto 24	—	—	—	—	—	1	50	24	17	03		
Ditto 25	8	50	24	17	05	—	—	—	—	—		
Ditto 26	8	45	24	15	44	1	45	24	19	02		
Ditto 30	—	—	—	—	—	1	50	24	27	04		
Ditto 31	—	—	—	—	—	1	55	24	24	00		

Mean of Observations in Dec.	Morning	at	8^{h}	53′.....	Variation	24°	17′	21″	West.
	Noon	at	1	53	Ditto	24	19	49	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	42	Ditto	24	17	42	West.
	Noon	at	1	54	Ditto	24	20	24	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	Ditto	24	15	41	West.
	Noon	at	1	59	Ditto	24	22	53	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	Ditto	24	15	46	West.
	Noon	at	2	02	Ditto	24	22	32	
	Evening	at	6	08	Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	Ditto	24	15	58	West.
	Noon	at	2	02	Ditto	24	23	32	
	Evening	at	7	05	Ditto	24	16	08	

Ditto in July.	Morning	at	8	37	..... Ditto	24	14	32	West.
	Noon	at	1	50	..... Ditto	24	23	04	
	Evening	at	7	08	..... Ditto	24	13	56	
Ditto in June.	Morning	at	8	30	..... Ditto	24	12	35	West.
	Noon	at	1	33	..... Ditto	24	22	17	
	Evening	at	7	04	..... Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	..... Ditto	24	12	02	West.
	Noon	at	1	37	..... Ditto	24	20	54	
	Evening	at	6	14	..... Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	..... Ditto	24	09	18	West.
	Noon	at	0	59	..... Ditto	24	21	12	
	Evening	at	5	46	..... Ditto	24	15	25	

Magnetical Observations continued.

1814.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Jan. 1	8h 55′	24° 16′ 32″	1h 55′	24° 19′ 06″		Discontinued, from the shortness of the days.
Ditto 2	8 55	24 16 17	1 55	24 17 13		
Ditto 3	8 55	24 12 48	1 45	24 15 35		
Ditto 5	8 55	24 12 36	1 45	24 15 50		
Ditto 6	8 55	24 15 50	1 55	24 15 40		
Ditto 7	8 50	24 14 39	1 50	24 17 04		
Ditto 8	8 55	24 11 39	1 55	24 15 42		
Ditto 9	8 55	24 13 20	1 50	24 16 47		
Ditto 10	8 55	24 14 24	1 55	24 17 28		
Ditto 11	8 42	24 16 00	1 55	24 22 09		
Ditto 12	8 55	24 16 48	1 58	24 19 55		
Ditto 13	8 55	24 14 55	1 45	24 19 12		
Ditto 14	8 55	24 15 50	1 55	24 19 10		
Ditto 15	8 55	24 15 38	1 55	24 20 14		
Ditto 16	8 55	24 13 10	1 55	24 18 44		
Ditto 17	8 55	24 15 08	1 55	24 21 10		

In taking the mean of the observations for the month of Dec. the observations made on the 3d and 30th are not included. During the foggy weather the needle was remarkable steady.

Rain fallen	Between noon of the 1st Dec. Between noon of the 1st Jan.	0·400 inches.

Article X.

On the Daltonian Theory of Definite Proportions in Chemical Combinations. By Thomas Thomson, M.D. F.R.S.

(*Continued from* Vol. II. p. 301.)

The nitrates, the composition of which I propose to exhibit in this essay, have been hitherto very imperfectly and inaccurately analysed. I have been obliged to make some new experiments on the subject. Guided by these, by the experiments of Berzelius, and by the laws of definite proportions, I flatter myself that the following table will furnish a much nearer approximation to the truth than any hitherto offered to the public.

The weight of an atom of azote which I chose in the *Annals of Philosophy*, vol. ii. p. 42, for the reasons there assigned, cannot, I find, be reconciled to the constitution of the nitrates. I have been obliged, in consequence, to make the following alterations in the weight of that atom, and in the weight of the bodies into which it enters.

	Number of atoms.	Weight of an integrant particle.
Azote		1·803
Nitrous oxide	1 *o* + 1 *a*	2·803
Nitrous gas	2 *o* + 1 *a*	3·803
Nitrous acid	3 *o* + 1 *a*	4·803
Nitric acid	5 *o* + 1 *a*	6·803
Ammonia	1 *h* + 1 *a*	1·935

According to these weights, nitrous gas must be a compound of 100 volumes of oxygen and 102·6 volumes of azote, instead of equal bulks.

GENUS II.—*Nitrates.*

		Number of atoms.	Weight of an integrant particle.
201.	Nitrate of potash	1 *n* + 1 *p*	10·082 [a]
202.	Nitrate of soda	2 *n* + 1 *s*	21·488 [b]
203.	Nitrate of ammonia	1 *n* + 1 *a*	8·868 [c]

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[a] Potash, when carefully dried, contains no water of crystallization. I find that 100 grains of dry nitre, when decomposed by sulphuric acid, give 83·6 grains of fused sulphate of potash, which is equivalent to 45·6 per cent. of base. Hence nitre is composed of 100 acid + 83·823 base. Dr. Wollaston's scale of equivalents gives us nitre composed of 100 acid + 86·764 base, which he informed me was the result of a synthetic experiment. Now 6·803 : 6 :: 100 : 88·1. This agrees nearly with Dr. Wollaston's result.

[b] 6·803 × 2 : 7·882 :: 100 : 57·92. Now Wenzel found this salt composed of 100 acid + 60 base. I have reason to believe that 100 acid + 57·8 base is very near the truth. Both of these correspond sufficiently with the numbers in the table. How Berthollet obtained, as the result of analysis, 102·44 acid + 100 base, I cannot conceive.

[c] This supposes the weight of an integrant particle of ammonia to be 1·935, and that it is composed of 1 *h* + 1 *a*. Now 6·803 : 1·935 :: 100 : 28·4; and Berzelius found the salt composed of 100 acid + 31·266 base (Gilbert's Annalen, 1812, p. 165). Of preceding analyses, Kirwan's seems to approach nearest the truth.

	Number of atoms.	Weight of an integrant particle.
204. Nitrate of magnesia ..	1 n + 1 m	9·171 [d]
205. Nitrate of lime	1 n + 1 l	10·423 [e]
206. Nitrate of barytes	1 n + 1 b	16·534 [f]
207. Nitrate of strontian ..	1 n + 1 str	13·703 [g]
208. Nitrate of ammonia-and-magnesia	4 n + 3 m + 1 a	36·251 [h]
209. Nitrate of copper	2 n + 1 c	23·606 [i]
210. Subnitrate of copper ..	1 n + 2 c	26·803 [i]
211. Nitrate of iron	2 n + 1 i	22·272 [k]
212. Pernitrate of iron	3 n + 1 i	30·075 [k]

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[d] According to this statement, we have 6·803 : 2·368 :: 100 : 34·808. Now I have reason to believe that the salt is composed of 100 acid + 34·729 base, which almost coincides with the statement in the table. Wenzel states the composition of the salt 100 acid + 38·88 base, which, considering the difficulty of the analyses, is by no means a bad result. All the other analysts deviate still farther from the truth.

[e] 6·803 : 2·368 :: 100 : 53·2. Now according to Kirwan's analysis the salt is composed of 100 acid + 55·70 base. I have reason to believe that the correct composition is 100 acid + 53·091 base.

[f] According to this statement 6·808 : 9·731 :: 100 : 143·03. Now according to the analysis of Berzelius the salt is composed of 100 acid + 140·73 base (Gilbert's Annalen, 1812, p. 165). I have reason to believe that the correct constituents are 100 acid + 142·71 base.

[g] This gives us 6·803 : 6·9 :: 100 : 101·42. Now Vauquelin's analysis gives us 100 acid + 98·347 base, which approaches very closely to the number in the table. I have reason to believe that the correct constituents of the salt are 100 acid + 101·193 base.

[h] This supposes the salt to be composed of 3 integrant particles of nitrate of magnesia and 1 integrant particle of nitrate of ammonia. This approaches nearest to the analysis of Fourcroy, though it does not agree with it.

[i] Both of these salts are known to exist; but they have not been subjected to analysis. I have reason to believe that the common nitrate of copper, when methods of analysing it have been discovered, will be found composed as the salt No. 209 in the table. The subnitrate has been analysed by Berzelius, who found it composed of 100 acid + 349·2 base. Proust obtained 16 acid + 67 oxide, or 100 acid + 418·7 base. The mean of these is 100 acid + 383·9 base, which is probably near the truth. The theoretical numbers are 100 acid + 386·67 base.

[k] It is well known that these two salts exist; though it would be scarcely possible to subject them to a rigid analysis. The numbers

		Number of atoms,	Weight of an integrant particle.
213.	Nitrate of zinc	1 *n* + 1 *z*	11·942 [l]
214.	Nitrate of lead	2 *n* + 1 *l*	41·580 [m]
215.	1st Subnitrate of lead.	1 *n* + 1 *l*	34·777 [n]
216.	2d Subnitrate of lead.	2 *n* + 3 *l*	97·528 [n]
217.	3d Subnitrate of lead.	1 *n* + 3 *l*	90·725 [n]
218.	Nitrate of nickel	3 *n* + 1 *n*	30·714 [o]
219.	Subnitrate of nickel	1 *n* + 7 *nick*	78·938 [p]
220.	Nitrate of silver	1 *n* + 1 *s*	20·421 [q]
221.	Nitrate of mercury	1 *n* + 1 *m*	32·803 [r]
222.	Pernitrate of mercury	1 *n* + 2 *m*	60·803 [s]
223.	Subnitrate of platinum	1 *n* + 4 *pl*	63·447 [t]

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in the table are theoretic, and calculated on the hypothesis that 100 nitric acid combine with a quantity of base which contains 14·666 oxygen. This I conceive to be the truth.

[l] This salt has never been analysed; but as it is a neutral salt, there can be little doubt that its constituents will be as in the table.

[m] Berzelius gives this salt composed of 100 acid + 205·1 oxide (Gilbert's Annalen, 1812, p. 166). This I conceive to be exact. It corresponds very nearly with the statement in the table.

[n] These are the three subnitrates of lead described by Berzelius in the *Annals of Philosophy*, vol. ii. p. 278. The first contains twice as much oxide as the octahedral nitrate of lead. It ought, in fact, to be considered as a nitrate, and the octahedral salt as a supernitrate. The second contains thrice as much oxide as the octahedral salt; and the third six times as much oxide.

[o] This statement agrees nearly with the analysis of Proust, who found nitrate of nickel composed of 100 acid + 45·45 base; but I do not put much confidence in the statement.

[p] This would be the result of Proust's analysis, if any confidence could be put in its accuracy. He found the salt a compound of 100 acid + 735·3 oxide.

[q] This statement I consider as more correct than any analysis hitherto published. According to it the salt should be composed of 100 acid + 200·1 base. Berzelius's analysis gives 100 acid + 216·45 base; and Proust's, 100 acid + 233·33 base: but neither of these results seems entitled to confidence.

[r] This is a theoretic result, for the salt has never been subjected to analysis.

[s] According to this statement the salt should be composed of 100 acid + 790·82 base. Messrs. Braamcamp and Siqueira-Oliva make it a compound of 100 acid + 733·33 base. This is a tolerably near approximation, considering the difficulty of analysing this salt.

[t] This approaches the analysis of Chenevix. He found the salt a compound of 100 acid + 809·09 base. Now 6·803 : 14·161 ×

	Number of atoms.	Weight of an integrant particle.
224. Nitrate of bismuth ..	1 n + 1 b	16·797 [u]
225. Nitrate of uranium ...	1 n + 1 u	21·803 [x]

The preceding list comprehends all the nitrates that have been subjected to any analysis by chemists. I conceive that arsenic, chromium, molybdenum, tungsten, columbium, and tin, are not susceptible of forming oxides capable of uniting with nitric acid. The new metals, palladium, rhodium, osmium, and iridium, have been hitherto obtained in too small quantities to admit of any regular analysis of the salts which their oxides form with acids. The nitrate of antimony possesses so little permanence that it would be scarcely possible to subject it to analysis. The nitrates of gold and cobalt have been often formed, though very little examined. The nitrates of manganese, tellurium, titanium, and cerium, have been hitherto but superficially examined. The nitrates of alumina, glucina, yttria, and zirconia, might have been given from theory if we had possessed any means of determining whether they contain one or two atoms of acid.

If the preceding table be considered as accurate, and I conceive it to be a tolerable approximation, then the proportions of oxygen in the acid and base which constitutes the different salts, will be as follows:—

	Oxygen in the acid.	Oxygen in the base,
Nitrate of potash	5	1
Nitrate of soda	10	2
Nitrate of magnesia	5	1
Nitrate of lime	5	1
Nitrate of barytes	5	1
Nitrate of strontian	5	1
Nitrate of copper	10	2
Subnitrate of copper	5	4
Nitrate of iron	10	2
Pernitrate of iron	15	3

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4 = 56·664 :: 100 : 832·9; so that the difference between theory and analysis does not exceed 3 per cent.

[u] According to this statement the salt should be composed of 100 acid + 145·42 base; for 6·803 : 9·994 :: 100 : 145·42. Now Berzelius found the salt to be composed of 100 acid + 142·69 base (*Lärbok i Kemien*, ii. 177).

[x] According to this statement the salt should be composed of 100 acid + 220·4 base. Now Bucholz states its constituents to be 100 acid + 232 base. This does not differ more from the theoretical result than might be expected in the analysis of so difficult a salt.

	Oxygen in the base.	Oxygen in the acid.
Nitrate of zinc	5	1
Nitrate of lead	10	2
1st Subnitrate of lead	5	2
2d Subnitrate of lead	10	6
3d Subnitrate of lead	5	6
Nitrate of nickel	15	3
Subnitrate of nickel	5	2
Nitrate of silver	5	1
Nitrate of mercury	5	1
Pernitrate of mercury	5	4
Subnitrate of platinum	5	8
Nitrate of bismuth	5	1
Nitrate of uranium	5	3

It will be seen, by inspecting the preceding table, that the rule of Berzelius, that the oxygen in the acid is a multiple of that in the base, holds in every case except eight. Several of these exceptions are sufficiently known to Berzelius; but it is not necessary to discuss the subject, as that Gentleman admits them, and makes use of them to prove that azote is not a simple body, but a compound containing oxygen. By this supposition he reduces the whole of the nitrates under his general law.

That azote is a compound body can scarcely be doubted. That it contains oxygen is probable, from its little combustibility. The weight which I have been obliged to assign to the atom of azote removes the objection which I stated on a former occasion to that supposition; for the weight 1·803 shows that it may be a compound of 1 atom oxygen + 1 atom of a base which weighs 0·803. These reasons render it by no means improbable that Berzelius's opinion may be well founded: but it is dangerous in chemistry to admit conclusions from mere theory; because no part of chemical theory is so well established as to furnish data independent of experiment. Hence azote must remain among the simple bodies, and the nitrates must constitute an exception to Berzelius's law, till some fortunate experimenter succeed in showing us the constituents of azote. Mr. Miers some time ago announced, in the *Annals of Philosophy*, that he had ascertained it to be a compound of hydrogen and oxygen, and promised to favour us with a detail of his experiments; but hitherto he has neglected to fulfil his promise.

I would not be understood, from what I have said here, to reject the law of Berzelius as inconsistent with chemical phenomena. The more I have examined it, the more correct, in general, does it appear; and it seems to promise to throw a new and lively light upon the nature of affinity: but it is proper to state and examine all the exceptions to any general law, whether real or apparent; because such exceptions never fail in the end to lay open to our view new secrets of the science under our consideration.

Though the numbers which I have chosen for azote and its compounds agree pretty well with experiment and with one another, I am not quite satisfied with them: some mystery still hangs over this intricate substance, which probably will not be removed till we become acquainted experimentally with its composition.

(*To be continued.*)

Article XI.

On Rain Water. By Mr. Stark.

(To Dr. Thomson.)

SIR,

I herewith send you extracts from a paper which I have recently read at the Norwich Philosophical Society, and should you think them worthy of a place in your *Annals of Philosophy*, they are at your service. My motive for making them public is to induce others who have more leisure for experimental research than myself to make further inquiries into the subject.

Your most obedient servant,

Norwich, Dec. 15, 1813. Wm. Stark.

I have long adopted the use of logwood (hamatoxylum campechianum) in preference to litmus, or any other of the vegetable colouring substances which are used as tests, finding it to be more powerfully acted on both by the alkalies and the acids.*

I was making some experiments on logwood during a violent thunder-storm, and exposed a few grains of it to the action of rain which fell at that time; the colour extracted was an orange red, the same as is produced by distilled water slightly acidified. This singular effect was so unexpected, that I doubted the accuracy of the experiment: a short time afterwards I repeated it, under similar circumstances, and found the same result.

On exposing logwood to the action of rain which fell when there had been *no thunder* for several weeks, a colour inclining to *lilac* was extracted; and water caught at the same time, at the height of 300 feet, produced also the lilac shade. These facts evidently indicated either the presence of lime, or some kind of alkaline matter.

I have frequently suspected the existence of such substances in the atmosphere, from observing colours that had been mordanted and dyed precisely the same, would, by exposure to the air, be sometimes reddened by the absorption of oxygen, or carbonic acid; at other times made bluer: this effect could not have been from any other cause than the action of lime, or an alkali.

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* Chevreul notices the delicacy of logwood as a test, Ann. de Chim. tom 81.

On my mentioning the circumstance of the lilac infusion to my ingenious friend Mr. Robert Higgin, he suggested the probability of its being caused by the presence of ammonia, which he supposed was formed by the combination of nitrogen and hydrogen in the atmosphere;* this might be absorbed and brought down by the rain in sufficient quantity to produce the effect.

I was so well satisfied with Mr. Higgin's hypothesis, that it was some days before I attempted to *prove* the cause of this extraordinary phenomenon; not believing that *lime* could ever exist in the atmosphere.

To ascertain what it really was that produced this surprising effect, I made the following experiments on the water:—

A quantity of it was heated in a retort, and a stream of muriatic acid gas made to pass into a receiver, in which the beak of the retort was placed; but no dense clouds were produced. On examining the water which had been heated, I observed a slight precipitate; and, in consequence, boiled the water till about half of it was evaporated, and *then* found a *copious* precipitate. Next, a quantity of water impregnated with carbonic acid was added to the water that had been boiled, which soon re-dissolved the precipitate: by expelling the carbonic acid by boiling again, it caused the same precipitate as before.

These experiments convinced me that the lilac colour which was extracted from the logwood, by the rain-water that fell when there had been no thunder for several weeks, was owing to the presence of super-carbonate of lime, which came down with the rain.

To prove what *acid* existed in the rain which fell during the thunder-storm, the following tests were used:—

A quantity of the infusion was boiled, and the colour became considerably bluer. By adding a small quantity of lime-water to the rain-water, a copious precipitate was produced. An inverted receiver, filled with water over the shelf of a pneumatic trough, had the beak of a retort introduced, which contained part of the acidified rain-water; a lamp was applied to the retort, and the water made to boil, which caused a considerable quantity of gas to be expelled; and on introducing a lighted taper, it was immediately extinguished.

These experiments prove satisfactorily that the *acid* found in the rain-water was the *carbonic*.

The experiments have been many times repeated with similar results.

For what purposes in nature a substance which is known to be destructive to animal life should accumulate to such an astonishing degree, it is difficult even to offer a conjecture. The formation of carbonic acid from the lungs of animals, &c. is very great, as has been proved by Mr. Ellis* and others; but all have neglected to prove what becomes of the carbonic acid so formed. It would seem, from its being found in a thunder-shower, that electricity has some influence in its modification.

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* I believe it is not *generally* admitted that these gases exist in their separate states in the atmosphere; but surely, when we consider the peculiar motion of meteors, the simultaneous occurrence of thunder, &c. there can be no doubts on the subject.

With respect to the existence of *lime* in the atmosphere,† it may be accounted for in two ways, (at least by hypotheses:)—1st. By evaporation. I believe it is now generally understood that about $\frac{1}{3}$ of the surface of the earth in this country is composed of lime; now, may not this be carried up in its most minute particles, combined with the vapour which is exhaled from the earth's surface, as pure lime, as a sub-carbonate, or as a super-carbonate? (most probably the latter.) The condensation of the vapour to form rain would also be a condensation of the particles of lime, which may be sufficient to account for the phenomenon mentioned.

According to one theory of evaporation, this is possible.‡

Or it may perhaps be *formed* in the atmosphere itself, if it be allowed that the gases exist in their separate states there.

It is well known that nitrogen and hydrogen form ammonia. If either of these substances be examined separately, no metaloid can be detected; but if analyzed in the state of ammonia, it can.§

The base of lime is a metal,‖ and some of its properties alkaline; therefore, may not it be formed by gaseous matter, aided by the electric influence?

The present state of science affords us but few proofs of the causes of many of the phenomena of nature; I have, however, ventured the above suggestions, for the want of better. Electricity seems to be the active operating agent, the grand modificator of matter; scarcely a change takes place but it is caused by its influence.

I hope this subject will be pursued by persons who reside in different parts of the kingdom, in order to ascertain whether the effects herein stated were caused by *local* circumstances. Should this not be the case, I shall, at some future time, offer remarks on the effects which such substances must necessarily have on the animal and vegetable economy.

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* Vide Inquiries, &c.

† When I first detected the lime, I was inclined to think that it might be caused by the finer particles being carried up from neighbouring lime-kilns, &c. by agitated air; and, in consequence, made the next experiment with water caught from the top sound-hole of the cathedral of this place, a height of 300 feet: whether this height would be sufficient to obviate such an effect must remain to be proved hereafter.

‡ Vide Dalton's Essay, Man. Memoirs, vol. 5.

§ Davy's Chem. Phil. p. 473.

‖ Ibid, p. 345.

ARTICLE XII.

ANALYSES OF BOOKS.

Essay on the Theory of the Earth. Translated from the French of M. Cuvier, Perpetual Secretary of the French Institute, Professor and Administrator of the Museum of Natural History, &c. &c. By Robert Kerr, F. R. S. & F. A. S. E. With Mineralogical Notes, and an Account of Cuvier's Geological Discoveries, by Professor Jameson. Edinburgh, Blackwood. London, Murray, Baldwin. 1813.

THIS is the most entertaining geological book which has hitherto fallen into my hands. The view which it gives of the subject possesses a great deal of novelty; it is supported by documents which leave no doubt respecting the accuracy of the most important positions. The author has been long known as a comparative anatomist of accuracy and learning. He has turned his pursuits into a new channel, and, by uncommon industry joined to great sagacity, and the fortunate situation in which he was placed, has been enabled to demonstrate the existence of the fossil remains of many animals which have disappeared from the earth at a period anterior to the most ancient history.

The rocks, which constitute the highest and most extensive chains of mountains upon the earth's surface, contain no animal remains. Hence they must have been formed before our planet was inhabited by vegetables or animals. But as these rocks are composed of beds placed in the most various and dislocated state with respect to each other, Cuvier considers it as clear that their position has been altered since their original formation, and that they have been elevated by some unknown agent from a considerable depth to ther present lofty position. In this opinion he agrees with Saussure, who, in an examination of the Alps, which was conducted with unwearied industry for a period of twenty years, pointed out the various positions of the beds, and endeavoured to demonstrate that they must have been elevated from a great depth.

In lower levels we find beds which have a more horizontal direction, and in which the remains of animals and vegetables may be traced. These remains belong at first to the lowest orders of animals and vegetables, but as we proceed downwards to lower and lower levels, we find the remains of more perfect animals; till at last, in the newest beds of all, we find the bones of animals, the species of which still exist upon the earth.

The great merit of Cuvier, as a geologist, consists in his examination of the fossil bones of quadrupeds, which exist in such abundance in different beds, and at great distances from each other. He has ascertained and classified the fossil remains of 78 different species of quadrupeds in the viviparous and oviparous classes.

Forty-nine of these are new species, hitherto entirely unknown to naturalists. Eleven or twelve have such an entire resemblance to species already known, as to leave no doubt of their identity; and the remaining sixteen or eighteen have considerable traits of resemblance to known species; but the comparison of these has not yet been made with so much precision as to remove all doubt. Of the 49 new species, 27 are classed under seven new genera; while the other 22 belong to 16 genera, or sub-genera, already known. Of these 78 species, 15 are animals belonging to the class of oviparous quadrupeds; while the remaining 63 belong to the mammiferous class. Thirty-two of these last are hoofed animals, not ruminant, and reducible to 10 genera; 12 are ruminant animals, belonging to two genera; seven are *gnawers*, referable to six genera; eight are carniverous quadrupeds, belonging to five genera; two are toothless animals, of the sloth genus; and two are amphibious animals, of two distinct genera.

These remains do not occur promiscuously, or huddled together in the same beds. Certain animals are always found in certain beds, the relative ages of which may be determined by their position. The oviparous quadrupeds are found in more ancient strata than those of the viviparous class. Thus the crocodiles of Honfleur and of England are found beneath the chalk, and the *monitors* of Thuringia occur in the copper-slate considered by Werner as one of the oldest of the floetz formations. The great alligators, or crocodiles, and the tortoises of Maestricht, are found in the chalk formation.

This earliest appearance of fossil bones seems to indicate that dry land and fresh water must have existed before the formation of the chalk strata. But mammiferous animals occur only above the chalk. We find the bones of the lamontin and seal in the coarse shell limestone which immediately covers the chalk in the neighbourhood of Paris; but no bones of mammiferous land animals are to be found in that rock. But in the formations that cover it they occur in great abundance.

Hence it would appear that the oviparous quadrupeds began to exist along with the fishes, and at the commencement of the period which produced the secondary formations; while the land quadrupeds did not appear upon the earth till long afterwards, and until the coarse shell limestone had been already deposited, which contains the greatest part of our genera of shells, although of quite different species from those which are now found in a natural state.

The coarse shell limestone lying above the chalk are the last formed beds, which indicate a long and quiet continuance of the water of the sea above our continent. The beds above them contain abundance of shells, but they are mixed with alluvial materials, and rather indicate violent transportations than quiet depositions. Where regular rocky beds appear above them, they generally bear the marks of having been deposited from fresh water. All the remains of mammiferous quadrupeds have been found either in these fresh-water formations, or in alluvial formations. The remains of all the unknown genera occur immediately above the coarse shell limestone, while the bones of those animals that still continue to exist upon the earth are only found in the very latest alluvial depositions.

From these and various other similar facts, which we have not room to notice here, Cuvier concludes that the surface of the earth has been repeatedly covered by the sea; that several successive races of inhabitants have been destroyed; and that all these revolutions took place before the continents which we at present know were inhabited by the human race. He does not mean to insinuate that mankind were not created till a period subsequent to these great catastrophes. They may have existed in some remote islands which were afterwards overwhelmed, and the few human beings who escaped the devastation may have afterwards spread themselves over the face of our present continents. But that man did not exist at the time of these revolutions in our continents he thinks evident from the well known fact that human fossil remains are nowhere to be found, though they are as well fitted for a lengthened existence as the bones of quadrupeds, birds, and fishes, which are found in abundance.

He shows, from a variety of circumstances, that the present human race cannot be of older date than the period assigned in the Old Testament to the deluge; that human history and establishments must have originated at that period; and that the traditions of the Egyptians, Assyrians, Indians, and Chinese, point uniformly to a deluge about that period, which must have swept almost the whole human race from the earth, and thus destroyed all civil institutions, learning, and civilization. Cuvier conceives, with Deluc, that, at the period of the deluge, the sea overwhelmed the ancient continents, and left its own bed uncovered. This change of situation accounts for the total want of human remains in our strata. These remains are buried at the bottom of the ocean, and will make their appearance if, in consequence of any similar future catastrophe, the bottom of the ocean be again laid bare.

Mineralogists will readily perceive that the beds which chiefly occupy the attention of Cuvier constitute the very latest of the formations. They in a great measure escaped the attention of Werner, no doubt in consequence of the nature of the country which he inhabits. The environs of Freyberg being primitive, such formations were not to be expected. They lie over the chalk, which constitutes one of the latest of the Wernerian formations. But now that they have been noticed and examined, there is every reason to expect that they will be traced in other parts of the world as well as France. Indeed, this opinion is already in some measure verified. Mr. Webster has shown that they constitute a considerable portion of the south-east of England. It has been announced, also, in one of the French journals, that they have been found in Silesia, in Spain, and in the South of France.

Hence it would appear that they exist over a very considerable proportion of Europe.

Article XIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, the 20th of January, was read a paper by Sir Humphry Davy, entitled, *Some Experiments and Observations on a new Substance which becomes a violet coloured Gas by Heat.* The substance in question was accidentally discovered, about two years ago, by M. Courtois, a manufacturer of saltpetre at Paris. In his processes for extracting soda from kelp he found his metallic vessels more corroded than they ought to have been. In endeavouring to discover the cause of this he obtained the new substance. The mode of obtaining it is easy. Concentrated sulphuric acid is poured upon the insoluble part of the kelp from which the soda has been extracted. Heat is produced, and the new substance appears under the form of a violet vapour, which condenses into crystals. M. Courtois gave specimens of it to MM. Desormes and Clement, who subjected it to experiments, and read a short paper on it at a meeting of the Institute on Nov. 29, 1813. They stated that its specific gravity was 4; that it becomes a violet coloured gas below the temperature of boiling water; that it combines with metals, phosphorus, and sulphur, with alkalies and metallic oxides, and forms a detonating compound with ammonia; that it dissolves in alcohol and ether; and that by the action of hydrogen and phosphorus on it an acid analogous to the muriatic is formed. Gay-Lussac had been supplied with a quantity of it by Clement, had subjected it to experiments, and had read to the Institute on the 6th December the memoir of which an abstract is published in the present Number of the *Annals of Philosophy*. Sir Humphry Davy was furnished with a quantity of it by M. Ampere, and was thus enabled to make experiments on it. The following is a pretty correct epitome of the facts stated by Sir H. Davy in his paper; the novelty of the subject having induced us to take some pains to state every thing with correctness:—

1. The new substance precipitates nitrate of silver lemon yellow. This precipitate fused at a low red heat, and became red. When acted on by potash, it was decomposed, and oxide of silver formed. Potash boiled upon it yielded the peculiar substance when treated with sulphuric acid. It was more rapidly acted on when exposed to light than muriate of silver, and was a distinct substance. When the peculiar substance is heated in contact with silver a similar compound is formed.

2. Potassium burns, when heated in contact with the vapour of the peculiar substance, with a pale blue flame. No gas was disengaged. A white substance was formed, fusible at a red heat, and soluble in water. Its taste was acrid. Sulphuric acid expelled from it the peculiar substance.

3. It combines with chlorine, and forms a yellow coloured solid, soluble in water, and rendering the liquid yellowish green and very acid.

4. In oxygen, or in contact with hyperoxymuriate of potash, it underwent no change.

5. It united with iron, mercury, tin, zinc, and lead, forming compounds fusible at a moderate heat, and almost all of fine colours. These compounds, when placed in contact of water, immediately evolved the respective metallic oxides. Several of them were capable of uniting with potash, and forming compounds from which sulphuric acid evolved the peculiar substance.

6. The combination of the peculiar substance and phosphorus is very analogous to phosphurane. During the combination acid fumes are given out. They are similar to the acid, formed more abundantly when the compound is heated in water. This acid is a gas, rapidly absorbed by water, and combining with bases, and forming peculiar salts. When mercury is heated in the acid gas the same compound is formed as that produced by the direct action of the peculiar substance on mercury, and hydrogen is evolved equal in bulk to half the bulk of the acid gas. Potassium produces a similar effect without any combustion taking place. All these, and other phenomena of the same kind, detailed in the paper, lead to the conclusion that the acid is composed of the peculiar substance and hydrogen.

7. It seems capable of uniting directly with hydrogen gas, and of forming the new acid.

8. It is dissolved in solution of potash, oxygen is expelled from the potash, and two new compounds are produced. One, precipitated in crystals, is a compound of the peculiar substance, potassium, and oxygen. It is similar in its properties to hyperoxymuriate of potash. The other, remaining in solution, is a compound of the peculiar substance and potassium.

9. Similar results are obtained with solutions of soda and barytes.

10. The peculiar substance is expelled from all its combinations by chlorine; while, on the contrary, it usually expels oxygen from its combinations.

11. When the peculiar substance is dissolved in liquid ammonia, a black powder precipitates, which detonates, and appears to be a combination of the peculiar substance and azote.

12. 2·8 grains of potassium are saturated by 6·25 of the peculiar substance. Supposing them to unite atom to atom, and that the weight of an atom of potassium is 5, then the weight of an atom of the peculiar substance will be 11·160: so that it is as heavy as several of the metals.

13. Mercury absorbs $\frac{2}{3}$ of its weight of the peculiar substance, and hence seems to combine with two atoms.

14. Sir H. Davy calculates the weight of the peculiar acid (on the supposition that it is a compound of equal bulks of the peculiar substance and hydrogen) condensed into half their bulk at 95·27 grains for 100 cubic inches.

15. The peculiar substance is not decomposed by Voltaic electricity. It is negative with respect to most substances, but with respect to chlorine it is positive.

16. We must consider it as an undecomposed substance analogous to chlorine and fluorine. Sir H. Davy proposes to call it *iodine*. The acid which it forms with hydrogen he calls *hydrionic*, the acid with chlorine *chlorionic* acid, that with tin *stannionic*. Its compounds with metals may be called *iodes*.

He concludes the paper by making another proposal as to nomenclature. To distinguish the combinations of fluorine by the letter *l*, of chlorine by the letter *n*, and of *iodine* by the letter *m*, the vowels representing the number of atoms. Thus *calca* is a combination of an atom of calcium and an atom of oxygen; *calcala*, a combination of an atom of calcium with an atom of fluorine; *calcana*, a combination of an atom of calcium with an atom of chlorine; *calcama*, a combination of an atom of calcium with an atom of iodine.

LINNÆAN SOCIETY.

On Tuesday, the 18th of January, the remainder of Mr. Marschal Von Biberstein's paper on the genus serratula was read. He described 18 species. He described, likewise, about 14 species of a new genus, distinguished by the name of *heterotrichum*, consisting of species removed from the genus serratula, on account of the different structure of the pappus.

At the same meeting a notice by Mr. Sowerby was read, respecting the *tremella meteorica*, a gelatinous matter ranked among vegetable substances. He requested information whether it was an animal or vegetable substance. He mentioned, at the same time, several other gelatinous substances, concerning the animal or vegetable nature of which doubts were entertained.

GEOLOGICAL SOCIETY.

At a meeting of the Society on the 17th of December, the continuation of Mr. Webster's paper on the upper strata of the S. E. part of England was read.

This part of Mr. Webster's paper begins by a description of the marine deposit which covers the lower fresh water formation in the Isle of Wight. The place where it may be studied to most advantage is Headen, near Alum Bay. It here appears about half way up the cliff, is about 36 feet thick, and dips a few degrees to the North. The substance composing the principal part of the bed is a pale greenish marl, filled with small shells chiefly cerethia, and cytherea and oysters, in a very perfect state of preservation. The extensive stratum containing shells, which appears at Woolwich, and in many other parts of the London basin, South of the Thames, are also considered by Mr. Webster as portions of the upper marine formation. Beds containing similar fossils, occur in the Paris basin, covering the gypsum and gypseous marls of the lower fresh water formation.

The above strata in the Paris basin, are covered by very extensive and thick beds of a pure sand, sometimes loose, sometimes concreted; with which is also connected that peculiar and valuable mineral, known by the name of *meuliere*, or *burr-stone*. In the Isle of Wight, there is nothing to correspond with these important beds except a thin layer of sand; but in the counties round London, there occurs in detached blocks, a very silicious sandstone called the *grey weathers*, which has been largely employed in architecture, and which is conjectured by Mr. Webster to be of cotemporaneous origin with the French sandstone.

The *upper fresh water formation*, one of the most remarkable and best characterized of any of the English beds, above the blue clay, is best seen at Hendon, in the Isle of Wight. Its thickness is about 55 feet, and though not subdivided into distinct strata, it varies considerably in texture. Much of it consists of yellowish-white marl, more or less indurated, but friable and crumbling by frost. Many of the shells imbedded in this stratum are quite entire, consisting of various species of lymneæ, planorbes, helices, and other fresh water shells. Over this bed is a stratum of clay with small bivalve shells, covered by a bed of yellow clay without shells, which latter is covered by a bed of friable calcareous sandstone, also without shells. To this succeed other calcareous strata with a few fresh water shells, varying much in compactness from that of chalk to porcellanous limestone.

This formation appears to have covered nearly all the northern half of the Isle of Wight.

In the Paris basin are strata corresponding with these both in their general composition and in the fossils which they contain, distinguished however by certain peculiar characters that are detailed by the author of this paper.

WERNERIAN SOCIETY.

At the first meeting of the sixth session of this Society, held on the 20th of November, the Secretary read a communication from Mr. Hisinger, of Sweden, containing an analysis of the variety of brown spar denominated *spath perlée*. He found that 100 parts contained

Lime	27·97
Magnesia	21·14
Oxide of iron	3·40
Oxide of manganese	1·50
Carbonic acid	44·60
Loss	1·39
	100·00

From the great similarity in their chemical composition, Mr. Hisinger objects to the ranking of sparry ironstone and brown spar under different genera, as Werner, from regarding only external characters, has been led to do.

At the meeting on the 4th of December a communication was read from Capt. Brown, of the Forfarshire Militia, containing descriptions and drawings of some rare and also of some new shells, found on the coast of Northumberland.

At the same meeting, Professor Jameson read a paper on conglomerated rocks. These, he remarked, occur in primitive, transition, and floetz country: the primitive conglomerates are conglomerated gneis, conglomerated mica slate, conglomerated granite, and conglomerated porphyry rock: the transition conglomerated rocks are greywacke, sandstone, and limestone; and the floetz conglomerates are sandstone and trap tuff. Mr. Jameson, from the crystalline character of these rocks, conjectures that all of them are original chemical deposites, and that therefore the quantity of mechanical matter on the crust of the earth is much less than is generally supposed.

At the meeting on the 8th of January, 1814, two communications from Capt. Laskey were read. The first gave an account of shells having been found in a bed of sand and clay, which was cut through in the line of the Ardrossan canal, near Paisley. This bed is situated about 40 feet above the present level of the Clyde. The shells found are chiefly the following: turbo littoreus, rudis, terebra; arca minuta, nucleus; patella pellucida, vulgata; buccinum lapillus, undatum: mytilus edulis; Venus islandica, striata, literata, pecten opercularis; balanus communis; anomia ephippium; tellina plana; nerita littoralis, glaucinum; mya truncata; trochus crassus; and carduum echinatum. They are generally somewhat worn or broken; but all of them are at this day to be found recent or alive in the frith of Clyde and its shores, at the distance, however, of 20 miles from this spot. The second communication described a fossil asterias found in a bed of sandstone near Aborlady: it seems most nearly allied to Asterias multiradiata.

Capt. Laskey also laid before the Society some specimens of wavellite in slate clay, from Loch Humphry, in Dunbartonshire. This is the second time this rare mineral has been observed in Scotland. It was first discovered in Scotland, in the isle of Glass, several years ago, by Mr. Neil, Secretary to the Wernerian Society.

At the same meeting, Professor Jameson read a series of mineralogical observations and speculations on stratification, veins, and coal. Mr. Jameson considers stratification as having been effected more by a simultaneous crystallization than by successive deposition, and that the seams of the strata are merely particular separations effected in the crystallizing mass, in the same manner as the seams are formed in distinct concretions, or the lamina in the slaty structure. Hence it follows that any two contiguous portions of granite are of simultaneous formation. The same must be the case with any two contiguous portions of granite and gneiss, or of gneiss and mica slate, &c. This view of stratification explains all the variation of appearance observed at the junction of rocks, as of granite with gneiss, &c. This idea in regard to stratification naturally leads to the opinion that many veins, hitherto considered as of *after formation*, are of *cotemporaneous formation*. Professor Jameson, by a statement of facts, rendered it probable that granite, porphyry, sienite, trap, and even metalliferous veins, are of cotemporaneous formation with the rocks in which they are contained, and also that veins may cross each other, and shift each other, and still be of cotemporaneous formation with the inclosing rock. From the view of the formation of strata and veins as given in this paper, it follows that the materials of which the solid mass of the earth is composed have been formed more simultaneously than is generally supposed, an opinion which is supported by the chemical formation of the strata, already proposed by Professor Jameson in his paper on conglomerated rocks. Coal has been generally considered as a vegetable production; and Professor Jameson used to consider this opinion as plausible. It would appear, however, from this paper, that he is now inclined to view glance coal and black coal as original mineral productions, bearing the same direction to their accompanying vegetable remains that limestone does to its accompanying shells and corals.

ARTICLE XIV.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Dr. Prout will commence a course of Lectures on Animal Chemistry on Friday, February 18, at half-past eight in the evening.

These lectures will be given at his residence, 4, Arundel-street, Strand, and will be continued weekly at the same hour.

II. *Preservation of Milk.*

The rapidity with which milk becomes sour in warm weather is well known. In a very short time it curdles, and becomes unfit for most of the uses to which it is usually applied. Sour milk, indeed, may be kept for a very considerable time without running into putrefaction. I once kept a phial of it for three years, merely stopped with a common cork. The curd had fallen to the bottom, and the whey was nearly transparent; but I could not perceive that it had undergone any further alteration after the first week. Kirchhoff, a Russian chemist, well known by his curious discovery of the method of converting starch into sugar, has proposed a method of preserving milk for any length of time, which is said to answer perfectly. He reduces it to a dry mass by gentle evaporation. This powder, when mixed with the requisite proportion of water, is brought back nearly to its original state. Eggs may be preserved by the same means.

III. *Meteorolite of Smolensk.*

Some time previous to 1810, a shower of stones fell at Smolensk in Russia. A specimen of one of them, analysed by Klaproth, was composed of

Iron	17·60
Nickel	0·40
Silica	38·00
Magnesia	14·25
Alumina	1·00
Lime	0·75
Oxide of iron	25·00
Sulphur, oxide of manganese, and loss	3·00
	100·00

IV. *Iberite.*

This is the name given to a mineral discovered, near Teflis in Georgia, in a floetz mountain composed of alternate beds of clay and sand. It was described in 1806, by M. Schlegelmilch, and the description published in 1810 in the 2d volume of the Memoirs of the Imperial Academy of St. Petersburg. It seems to be connected with the family of zeolites; but as I have never seen a specimen, and no analysis of it has been yet published, it would be premature to determine whether or not it be a new species. Schlegelmilch's description of it is as follows:—

Colour, snow white. Found massive and crystallized:—1. In oblique four-sided prisms, with the terminal faces obliquely truncated. 2. In needles. The crystals are usually small, or very small, and form druses. Surface smooth. External lustre glimmering, or nearly dull; internal, intermediate between splendent and shining. The kind of lustre is silky, approaching to pearly. Fracture radiated; the radii short and straight. Longitudinal fracture foliated, and seemingly with a double cleavage. Surface of the fracture longitudinally fine streaked. Fragments indeterminate, with rather blunt edges. Small fragments usually splintery. Distinct concretions, longitudinal grains, sometimes nearly scapiform. Opake, or simply translucent, on the edges. Very soft. Moderately brittle. Breaks easily. Adheres a little to the tongue. Feels meagre. Moderately heavy.

Small pieces of it, when plunged into water, become transparent. It does not phosphoresce when scratched in the dark. This distinguishes iberite from tremolite.

Before the blow-pipe it swells a little, and melts with difficulty into a white transparent glass. With borax, it melts easily into a glassy bead. It dissolves, likewise, in soda, but with more difficulty. It does not effervesce with acids. It dissolves imperfectly in muriatic and nitric acids; and with the last acid it forms a jelly.

V. *Variation of the Magnet.*

M. Huth found the declination of the needle at Kharkoff, in Russia, 5° 4′ west, and its inclination, or dip, 66° 15′, in the year 1809.

VI. *Origin of the North American Indians.*

M. Julius Von Klaproth has made a curious discovery respecting the American Indians. He has found a long chain of nations and idioms extending from the canal of Queen Charlotte along the north-west coast of America, to Southern Canada, the United States of America, Louisiana, Florida, the Great and Little Antilles, the Caribee islands, and Guiana, as far as the river of the Amazons, where the languages and idioms are all obviously derived from an original language, which has a great deal of affinity with that of the Samojedes and Kamptchadales. The people all along this vast track, both in their figure and mode of life, have a striking similarity to the free nations in Northern Asia. Mr. Klaproth gives a list of Caribee words which occur in the languages of the Mandshou, the Samojedes, the Korgacks, the Youkaguirs, the Toungouses, the Kamtchadales, the Tchouktchis, &c.

VII. *Substitute for Tea.*

The inhabitants of the government of Irkoutsk, in Russia, are in the habit of drinking a tea which they prepare from the leaves of the following plants:—Saxifraga crassifolia, pyrola rotundifolia, clematis alba, pyrola uniflora, prunus padus, spiræa coronata, ulmus campestris, polypodium fragrans, rosa canina. Of these the pyrola rotundifolia, (*winter green,*) prunus padus, (*bird cherry,*) ulmus campestris, (*common elm,*) and rosa canina, (*common dog rose,*) are natives of Great Britain. The last two indeed are abundant all over the island.

VIII. *Miaszite.*

Dr. Wuttig, Professor of Chemistry and Mineralogy at the Russian University of Kazan, has announced the discovery of a new mineral, to which he has given the name of miaszite. According to his statement, it is a compound of carbonate of strontian and carbonate of lime. If Stromeyer's discovery, that arragonite is composed of these two carbonates, be verified, this Russian mineral ought to be a variety of arragonite, or, at least, connected with it.

IX. *Gum in Lichens.*

Kirchhoff has subjected the lichen esculentus, and lichen coralloides, to a chemical examination, in order to determine the proportion of gum contained in each. The first yielded 13, and the second 14 per cent., of a brown transparent gum, similar in its properties to gum arabic, and capable of being applied to the same uses.

X. *Extraordinary Fog.*

Between Monday, the 27th of December, and Sunday, the 2d of January, 1814, a most extraordinary fog prevailed in London, and it seems to have extended a great many miles round in every direction. It was frequently so thick that it was impossible to see across the street. Candles were burnt in most of the shops and counting-houses all day long. The mean temperature of the air during the week was 27°. The thermometer was never higher than 34°, nor lower than 22·5°. The fog condensed upon the grass, the trees, and every wooden or iron railing. The grass in consequence was covered with a coating of snow at least half an inch thick. Below the trees in St. James's park there lay a bed of snow at least an inch thick, which had fallen down from them. In London, the thickness of the fog was still farther increased by the smoke of the city; so much so, that it produced a very sensible effect on the eyes, and the coal tar vapour might be distinctly perceived by the smell. But at a distance from town, though there was no smoke, the fog was very thick. Not a breath of wind was perceptible during the whole week.

To account for the existence of such a fog, at such a temperature, and for such a length of time, constitutes a very difficult meteorological problem. According to the best data, air at the temperature of 27° is capable of containing a quantity of vapour weighing $\frac{1}{44500}$ of the atmosphere; and this quantity produces perfect saturation. Were we to suppose the whole atmosphere to be saturated with vapour, and this vapour, by some means or other, to be condensed into particles of water, its quantity is so small that it could scarcely constitute a thick fog; since, allowing it all to be deposited on the surface of the earth, it would not amount to three quarters of an ounce avoirdupois for every square foot. The quantity of vapour condensed into snow upon the surface of the earth during the fog must have exceeded this very considerably. And this estimate, small as it is, must be reduced at least to one half, because there is no reason to believe that the upper half of the atmosphere contains vapour.

Were we to suppose a constant stream of warm air from the south flowing over our atmosphere at some height above the surface, it would have produced clouds, and these clouds might have fallen down to the ground and constituted a fog. But I cannot conceive the existence of such a current over still air for a week, and a constant precipitation of vapour, without producing a sensible increase of the temperature of the place. No such change, however, took place to any great degree. On Sunday, the 9th, a slight wind began to blow from the east, which dissipated the fog. It increased on Monday, Tuesday, and Wednesday, and occasioned a heavy fall of snow.

Nothing can be a more striking proof of the little progress hitherto made in meteorology, than the difficulty of proposing a legitimate explanation of a phenomenon so common and familiar as a thick fog during winter.

XI. *Queries by a Correspondent.*

I have received the following queries from a correspondent, who subscribes himself *Inquisitor Imperitus,* and who requests an answer to them.

1. Have any analyses been made of ancient mortars and cements? The absorption of carbonic acid cannot, as is said, be the cause of their hardness and durability; at least, it remains to know what favours the production of carbonic acid in mortar. For age, which has produced that admirable degree of hardness in ancient mortars, has irretrievably mouldered and crumbled the mortar of other buildings not two hundred years old.

Answer. Ancient mortars have been frequently analysed, and found to consist of nothing else than sand and lime; and as good mortar can be made in the present day as the ancient mortar. I have seen houses 100, 200, 300, years old taken down, in which it was easier to break the stones than the mortar. The treatise on mortar by Higgins gives, I conceive, directions which would enable any person to make excellent mortar, if he chuses to follow them. Mortar owes its solidification not to the absorption of carbonic acid, but to the combination of water with the lime. When the lime contains clay, it even hardens under water, though carbonic acid has no access to it. I have seen lime-water made from a piece of ancient mortar; a proof that it had not absorbed carbonic acid at all. I do not know any thing of the history of that mortar. The specimen was sent me from Galloway in Scotland.

2. What is the composition of Parker's cement?

Answer. If I recollect right, it is composed of clay iron-stone, and lime, beaten together. But perhaps I am mistaken.

3. Is there any difference between sulphate and carbonate of lime, when perfectly calcined?

Answer. A very great difference. Carbonate of lime, by calcination, parts with its carbonic acid, and is reduced to quick lime. Sulphate of lime only parts with water, and still continues a compound of sulphuric acid and lime.

4. What is the menstruum of caoutchouc, and how are elastic gum bougies formed? Not certainly by welding one longitudinal stripe to another, as is represented!

Answer. The only known solvent of caoutchouc is ether washed in water. I have no practical knowledge of the method of preparing bougies,

5. Is there any certain method of detecting barley or potatoe flour in wheaten bread? When wheaten flour is dear, bread contains 1-4th or 1-5th of that from barley or potatoes. Now it is hard to pay four-pence or five-pence for a pound of what might be had for one penny in the shape of potatoes.

Answer. The best criterions for detecting the presence of these substances is the colour, taste, and appearance of the loaf. If it be hard, black, and doughy, the presence of some improper article may be pretty confidently suspected.

6. Of what use is lime in the smelting of iron, as stated in Dr. Thomson's Travels in Sweden?

Answer. Lime is always used by the smelters of iron. It has the property of forming a glass with the clay which is usually mixed with the ore, and thus separates it from the iron. I believe that, in some of our founderies, too much lime is often used.

XII. *Test for Arsenic.*

In consequence of a paragraph in the last number of the Annals of Philosophy, I have received a letter from Mr. Hume, of Long Acre, claiming the original discovery of the application of nitrate of silver as a test of the presence of arsenic when in solution, accompanied by all the documents on which that claim is founded. I have perused them, and find the following to be the facts. The test was first proposed by Mr. Hume, and his description of the application is sufficient to show us that he understood the principle upon which it acted; though he does not explain that principle in his letter published in the Philosophical Magazine for 1809. It was easy for any chemist to apply that principle. Dr. Marcet's mode of using the test I consider as an improvement, though I have no doubt it was suggested by Mr. Hume's letter. As to Mr. Hume's last method, I have never tried it; but see no reason to doubt that it will answer.

XIII. *Iodine.*

I have made some trials on the production of iodine, and conceive it right to state the results that I have obtained, as it may save others some needless trouble. Pounded kelp did not yield any of it. When kelp is treated with water till every thing soluble is taken up, the insoluble residue I found chiefly carbonate of lime. It did not yield any iodine when treated with sulphuric acid; but the dry salt extracted from kelp by water yields it in abundance. My method of proceeding was as follows: Take a tubulated retort, rather deep, but not large, and having a short neck; fix its beak into a large globular glass receiver, (the larger the better,) leaving room for the air to escape; put the dry salt into the retort, pour strong sulphuric acid on it through the mouth of the retort, and then close it with a stopper. A violent effervescence ensues; the violet gas is driven off in abundance. It crystallizes upon the inside of the receiver, and may be washed out with water. The water employed for this purpose usually has a yellow colour, and contains the acid formed by the union of chlorine and iodine.

XIV. *Meteorological Table: extracted from the Register kept at Kinfauns Castle, North Britain.* Communicated by the Right Hon. Lord Gray.

Supposed Lat. 56° 18′.—Above the Sea 90 feet.

1813.	Morning, 8 o'clock *Mean height of* Barom.	Morning, 8 o'clock *Mean height of* Ther.	Even. 10 o'clock. *Mean height of* Barom.	Even. 10 o'clock. *Mean height of* Ther.	Dep. of Rain. In : 100	No. of days, Rain or Snow.	No. of days, Fair
January	30·07	33·00	30·09	33·70	1·05	10	21
February	29·59	38·00	29·59	38·00	2·66	23	5
March	30·07	41·90	30·08	40·90	·41	14	17
April	29·99	43·40	30·04	40·97	·73	11	19
May	29·84	49·06	29·85	46·93	2·50	22	9
June	30·10	54·83	30·10	53·00	1·07	9	21
July	29·89	57·90	29·88	56·50	2·44	13	18
August	30·09	56·51	30·09	54·16	·63	5	26
September	30·05	52·40	30·04	52·10	1·72	10	20
October	29·82	42·50	29·81	43·00	1·70	10	21
November	29·71	37·50	29·72	37·60	1·47	11	19
December	29·92	37·51	29·89	37·71	·95	12	19
Aver. of the Year.	29·929	45·375	29·934	44·547	17·33	150	215

ANNUAL RESULTS IN 1813.

BAROMETER.

	At 8 o'clock, A.M.	*At 10 o'clock, P.M.*
Highest,	Jan. 22, Wind S.W. ....30·58	Jan. 31, Wind N.W. ....30·57
Lowest,	April 1, —— S.E. ....28·74	Feb. 17, —— S. ....28·73

THERMOMETER.

Highest,	Aug. 12, Wind S.E. ......64°	July 30, Wind S. ....63°
Lowest,	Jan. 26, —— S.W.......17	Jan. 25, —— W. 19

By one of Barbon's best double tube Thermometers.

Highest observation, afternoon of July 30, Wind S. 72°
Lowest observation, morning of Jan. 26, ——S.W. 16
Mean temperature for the year 1813................ 46·83

Weather.	*Days.*
Fair	215
Rain	150
	365

Wind.	*Times.*
N. and N.E.	10
E. and S.E.	76
S. and S.W.	101
W. and N.W.	178
	365

By observation at 8 o'clock in the morning.

Rain.

In : 100.
The greatest fall in 24 hours, July 16, Wind W...........0·78
The greatest in one month, in Feb.......................2·66
The least ——————, in March ....0·41
Total quantity fallen at Kinfauns Castle in 1813 ....17·33

N.B. The latitude is only assumed, and may be a few minutes more or less when ascertained by observation.

Article XV.

List of Patents.

Isaac Wilson, Bath, Gentleman; for certain improvements upon stove grates, to prevent smoky rooms, and for obtaining an increased heat from the same quantity of fuel. Nov. 29, 1813.

Maurice de Jouch, Kentish Town, Middlesex; for a method of preparing madder roots and madder. Nov. 29, 1813.

John Cragg, Esq. Liverpool; for certain improvements in the facing of exterior and interior walls of Gothic or other structures, built of brick or other material, with strong milled or sawn slates, bound and secured by mouldings, grooves, and tyes of cast-iron, in such a manner as to have the appearance, when sanded, of finely wrought stone-work in ornamental pannels, or otherwise; with ceilings of correspondent tracery, form, and character, of the same materials, which may be supported by pointed arches, rising from single or clustered columns of cast-iron or otherwise; and in capping buttresses in Gothic architecture with highly enriched pinnacles of cast-iron only, the which being connected by metal, with the spouts also of metal, and carried down to the ground, form conductors for the protection of lofty buildings from the effect of lightning; also for a spiral stain (wholly of cast-iron) of a light and simple construction, which may be carried up or inserted within the corner of a buttressed tower, or wall, or in the cylinder of a small turret; by which mode of facing, adorning, and constructing, the said several parts, churches, or other buildings, of pure Gothic design, may be erected of brick, and finished with light ornamental carved work, of appropriate taste and elegance, at less expense than if wrought on stone, and in materials that will endure. Nov. 29, 1813.

Samuel Tyrrel, Peddinghoe, Sussex, farmer; for a broadcast sowing machine. Dec. 4, 1813.

Article XVI.

Scientific Books in hand, or in the Press.

Mr. Robertson Buchanan, author of Essays on the Economy of Fuel, has in the Press a Practical Treatise on Mill-work and other Machinery.

Mr. J. Bankes has in the Press a Treatise on the Diseases of the Liver, and Disorders of the Digestive Functions, with admonitory Hints to Persons arriving from Warm Climates.

Dr. Benjamin Heyné, who has resided many years in India, in the confidential service of the East India Company, is about to publish in one volume, 4to. Tracts Historical and Political relative to the Carnatic, with a Voyage to Sumatra, &c.

ARTICLE XVII.

METEOROLOGICAL JOURNAL.

1813.	Wind.	Barometer. Max.	Barometer. Min.	Barometer. Med.	Thermometer. Max.	Thermometer. Min.	Thermometer. Med.	Evap.	Rain.	
12th Mo.										
Dec. 14	N W	29·93	29·87	29·900	35	25	30·0			☾
15	N W	29·87	29·49	29·680	38	26	32·0			
16	S W	29·49	29·35	29·420	49	40	44·5		—	
17	S	29·35	29·14	29·245	51	50	50·5	2	·19	
18	S W	29·16	29·14	29·150	54	44	49·0		—	
19	S W	29·28	29·16	29·220	50	35	42·5			
20	W	29·57	29·28	29·425	45	31	38·0			
21	N E	29·50	29·44	29·470	46	32	39·0	5	·12	
22	S W	29·77	29·50	29·635	47	36	41·5			●
23	S W	29·89	29·77	29·830	47	38	42·5		—	
24	S W	30·02	29·89	29·955	53	46	49·5		—	
25	S W	30·28	30·02	30·150	51	41	46·0	1	9	
26	N W	30·49	30·28	30·385	41	28	34·5			
27		30·49	30·38	30·445	31	25	28·0			
28		30·35	30·31	30·330	30	24	27·0			
29		30·36	30·35	30·355	30	19	24·5			
30	N W	30·36	30·32	30·340	32	22	27·0			☾
31	N	30·32	30·17	30·245	35	22	28·5			
1814										
1st Mo.										
Jan. 1		30·17	29·86	30·015	31	20	25·5			
2		29·86	29·71	29·785	32	28	30·0			
3		29·71	29·60	29·655	33	29	31·0			
4	E	29·60	29·20	29·400	33	25	29·0			
5	N E	29·12	29·08	29·100	33	32	32·5			
6	N	29·52	29·12	29·320	34	15	24·5			○
7	N W	29·66	29·60	29·630	28	11	19·5			
8	N W	29·6[illegible]	29·62	29·665	31	12	21·5			
9	N W	29·79	29·65	29·720	29	8	18·5			
10	N W	29·89	29·79	29·840	26	21	23·5			
11	S E	29·89	29·54	29·715	25	15	20·0			
12	N	29·88	29·49	29·685	27	15	21·0	6	1·28	
		30·49	29·08	29·757	54	8	32·36	0·14	1·68	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Twelfth Month.—14, 15. Hoar frost: clear days. 16, 17. Cloudy: rain at intervals: bees quit the hive in unusual numbers for the season. 18. Windy: some rain. 19. Very misty, a.m. 20. Hoar frost. 21. The same, followed by wind and rain. 23, 24, 25. Misty: drizzling rain at intervals. 26. A clear morning, with much dew: the barometer rising fast: *Cirrus* and *Cirrostratus*: orange coloured twilight. 31. Since the 26th we have had a succession of thick fogs, with a calm air, or at most a breeze from the N.E. Yesterday the air cleared a little: and to-day has been fine. A display of *Cirrus* cloude, with much red in the morning and evening sky. The peculiar smell of electricity has been perceptible of late, when the air cleared up at sun-set.

1814. *First Month.*—4. The mists, which have again prevailed for several days, and which have rendered travelling dangerous, are probably referable to the modification *Stratus*. The air has been, in effect, loaded with particles of freezing water, such as in a higher region would have produced snow. These attached themselves to all objects, crystallizing in the most regular and beautiful manner. A blade of grass was thus converted into a pretty thick *stalagmite*: some of the shrubs, covered with spreading tufts of crystals, looked as if they were in blossom; while others, more firmly incrusted, might have passed for gigantic specimens of white coral. The leaves of evergreens had a transparent varnish of ice, with an elegant white fringe. Lofty trees, viewed against the blue sky in the sunshine, appeared in striking magnificence: the whole face of nature, in short, was exquisitely dressed out in frost work. When the sun, at length, broke through, and loosened the rime, it fell unmelted, and lay in heaps under the trees; after which a deep snow, brought by an easterly wind, reduced the whole scenery to the more ordinary appearances of our winter. 5. Snow early, and during the day: the wind increasing in force from the N.E. 6. A dark morning. Snow falling in some quantity to-day, with the temperature at the surface 33° or 44°, presented an amusing phenomenon, which was pointed out by my children. Instead of driving loose before the wind, it was collected occasionally into a ball, which rolled on, increasing till its weight stopped it: thousands of these were to be seen lying in the fields, some of them several inches in diameter. 9. A somewhat misty morning: the snowy landscape, visible to the distance of about a mile only, exhibited a bluish tint. A thermometer, *placed on the snow*, fell this night to 6°: and I am informed that at Croydon a temp. of 5° was observed by a thermometer at the usual elevation from the ground: the time 11 p.m. 11. Very red sun-rise: a steady breeze at S.E. till evening. Min. tem. on the snow 11°. 12. *Cumulus* and *Cirrostratus* clouds. Min. tem. on the snow 12°. The river Lea is now firmly frozen, and the Thames so much encumbered with ice that navigation is scarcely practicable.

RESULTS.

Prevailing Winds, Southerly in the fore part, and after a calm interval Northerly.

Barometer:	greatest height	30·49 inches;
	Least	29·08 inches;
	Mean of the period	29·757 inches.
Thermometer:	Greatest height	54°
	Least	8°
	Mean of the period	32·36°
Evaporation		0·14 inch.
Rain, (with the products of melted snow and rime)		1·68 inch.

Tottenham, *First Month*, 17, 1814. L. HOWARD.

ERRATA IN THE LAST JOURNAL.

For *insulated*, read *inosculated*: for *epasm*, read *spasm*: for *a production*, read *it productive*.

ANNALS

OF

PHILOSOPHY.

MARCH, 1814.

ARTICLE I.

Biographical Account of Mr. Tobias Lowitz, Member of the Imperial Academy of Sciences of Petersburg.*

MR. Tobias Lowitz, member of the Imperial Academy of Petersburg for the department of chemistry, counsellor of state, and knight of the 2d class of the order of St. Anne, died of an apoplectic fit, on the 25th of November, 1804, in the 48th year of his age. He was born in 1757, at Gottingen, where his father was a professor in the university. His father, being an astronomer of some note, was invited by the Petersburg academy to observe the transit of Venus in 1769, and young Lowitz accompanied him to Petersburg in 1767. He went with his father to the southern provinces of the Russian empire, and was the melancholy witness of the tragical death of the author of his being, who was taken and impaled by the rebels at Pugatcheff.

He himself fortunately escaped, and returned to Petersburg, in 1775, with the wreck of the astronomical expedition. Here he was placed, as an eleve of the crown, in the gymnasium of the academy, where he received the first part of his academical education. His tutor, M. Guldenstedt, having the sagacity to discover the kind of study to which he had the greatest inclination, placed him as an apprentice in the great Imperial Apothecary's Hall; and, when he had finished his apprenticeship, sent him to Gottingen to prosecute his studies, and if possible to remove that melancholy which had been produced by the dreadful death of his father almost before his eyes.

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* Translated from the *Mémoires de l'Académie Imperiale des Sciences de St. Petersburgh*, tome 1. Published at Petersburgh in 1810.

An eager desire to visit the most interesting parts of Europe induced him, after he had completed his university education, to undertake a journey through Germany, Switzerland, Italy, and part of England. He formed the resolution to travel on foot, partly from motives of economy, and partly because he thought that the exercise would contribute to the re-establishment of his health. He returned to St. Petersburg in 1784, quite a new man. He was soon after appointed superintendant of the laboratory belonging to the Imperial Apothecary's Hall; and, in this situation, his talent for experimenting speedily developed itself. Some interesting discoveries in pharmaceutic chemistry, the forerunners of those which afterwards rendered his name so celebrated, having been made known to the academy, he was received in 1787 among the number of Correspondents, a distinction which was speedily followed by a pension. He was admitted into the number of Adjuncts in 1790, and was named an ordinary academician for chemistry in 1793.

The number and importance of his discoveries having given celebrity to his name, various academies and learned societies enrolled him in the lists of their members. The Russian government endowed him with honorary distinctions and civil advancements. He passed rapidly from the situation of counsellor of the court and counsellor of the college, to that of counsellor of state; and in 1801 he was decorated with the second class of the order of St. Anne.

Afflicted by the tape worm, and deprived during the last years of the use of his left hand, in consequence of the fall of the glass-covering of his mineral cabinet, which had cut the tendons and nerves of his fore-arm, his life became irksome and disagreeable. The only enjoyment that he ever possessed was derived from his chemical discoveries. His uprightness, his sweetness of temper, his knowledge and his misfortunes, naturally drew the interest of all his acquaintance. His successful labours will cause his name to be long cherished by all the friends of science, and particularly by the academy, of which he was one of the greatest ornaments.

APPENDIX BY THE EDITOR.

I have thought it worth while to translate the preceding short biographical notice of Lowitz, on account of his great merit as a chemist, and the celebrity which he acquired. To give the reader some idea of his scientific labours, I shall subjoin a list of such of his dissertations as I have had an opportunity of seeing. Considering the nature of the warfare in which this country has been engaged during the greatest part of his career, and our diminished intercourse with the continent, during a considerable part of it, I think it very likely that he may have published several papers which have not come to my knowledge.

1. A new method of concentrating vinegar and reducing its acid to a solid and crystallized state, published in the *Nova Acta* of the Petersburg Academy, vol. vii. p. 330. This curious paper was inserted in the English translation of Crell's Annals, edited by Dr. Richard Pearson, and a collection of very great merit.

2. A continuation of the dissertation on the crystallization of vinegar exhibiting various newly discovered methods of accomplishing it. *Nova Acta,* t. viii. p. 316.

3. Observations on a method of crystallizing common salt by cold, and of bringing it to a state of purity. Ibid. p. 364.

4. A description of a remarkable meteor seen at Petersburg on the 18th June, 1790. Ibid. p. 384.

5. New experiments on the crystallization of the caustic alkalies. *Nova Acta,* t. ix. p. 311. This paper was inserted in one of the early numbers of Nicholson's Journal.

6. A report on the metallization of the earths, inserted in the historical department of the same volume of the *Acta,* p. 32. Lowitz repeated the experiments of Ruprecht and Tondi, and found their results erroneous, as had been done by other chemists.

7. A new method of purifying putrid water. *Nova Acta,* t. x., p. 187. This was by means of charcoal. The Russians, in consequence of this paper, charred the inside of their casks, and found that water might be kept in them in that state, at sea, for any length of time without becoming putrid.

8. Strontian detected in ponderous spar as a secondary constituent. Ibid. p. 321. This is an excellent paper, in which the properties of strontian are described at great length. It would appear that Lowitz obtained muriate of strontian accidentally, before he was aware of the experiments made by other chemists on this new earth.

9. An exposition of a set of new observations respecting the crystallization of salts, and an account of a new method of obtaining more regular crystals than are usually procured. *Nova Acta,* t. xi. p. 271. The method is to put some of the dry salt into a saturated solution and to set it aside.

10. Experiments on the method of reducing alcohol to the greatest possible state of concentration. Ibid, p. 299. The method which Lowitz proposes is to dissolve potash in them. But Richter's method, by means of muriate of lime is cheaper and easier.

11. New experiments on artificial cold. *Nova Acta,* t. xii. p. 275. He used first potash and snow, but afterwards found that muriate of lime and snow answered nearly as well. These experiments were copied into Nicholson's Journal from the Annales de Chimie.

12. A chemical analysis of the hyacinths of Siberia, discovered by Laxmann. Ibid. p. 300. The result of the analysis was as follows:

Lime	41
Silica	35
Alumine	13
Iron	6
Water	1
Loss	4
	100

It is needless to observe, that these are not the constituents of the true hyacinth.

13. A chemical analysis of the topaz of Siberia. *Nova Acta*, t. xii. p. 406. He obtained,

Silica	46·15
Alumine	46·15
Water	0·69
Loss	7·01
	100·00

This, supposing the loss to have been fluoric acid, is an approximation to the analysis of Klaproth.

14. A new method of forming super-carbonate of potash, with new observations on the carbonate of potash. *Nova Acta*, t. xiii. p. 257. The method is this: dissolve potash of commerce in water, pour distilled vinegar into the solution till an effervescence just begins to appear, then set the liquor aside. The super-carbonate of potash crystallizes, and may be separated by the filter. Afterwards acetate of potash may be procured by proper evaporation.

15. A new method of obtaining pure tartaric acid from tartar. *Nova Acta*, t. xiv. p. 343. The method is as follows: mix 15 parts of crude tartar, 4 parts of chalk, and 100 parts of water. When the effervescence is at an end, bring the liquor to a boiling heat, then add muriate of lime in small portions at a time, as long as any precipitate falls. Decant off the liquid portion, edulcorate the tartrate of lime, and treat it with 8 parts of sulphuric acid, diluted with its own weight of water. Digest for two days, then filter and evaporate the liquor till tartaric acid crystallizes.

16. Some new remarks on the nature of honey, and on the method of bringing its saccharine constituent to a dry state. *Crell's Annals*, 1792, t. i. p. 18.

17. On the purification of nitre by means of charcoal and alum. Ibid, t. ii. p. 506.

18. On the combustion of metallic sulphurets without the contact of oxygen gas. Ibid. 1796, t. i. p. 239.

19. A method of freeing sulphuric ether more completely from alcohol than has hitherto been done. Ibid. p. 429.

20. Some chemical remarks in a letter to Crell. Ibid, 1797, t. i. p. 480. He describes Kirchhof's method of obtaining barytes from

the sulphate the moist way, and mentions Kirchhof's method of preparing cinnabar.

21. Some observations on titanium. Ibid, 1799, t. i. p. 183. This paper contains an analysis of a combination of titanium and iron, with remarks on the best method of separating the two substances from each other.

22. Notice of a new, convenient, and expeditious method of dissolving minerals in potash. Ibid. 1799, t. ii. p. 283. I believe this paper is published in Nicholson's Journal.

23. An easy method of dissolving silica in potash the moist way. Ibid. p. 378.

24. Remarks on the way in which charcoal acts as a purifier. Ibid. 1800, t. i. p. 191.

25. A cheap and profitable method of bringing wine and beer vinegar to the state of glacial acetic acid. Ibid. p. 291.

I have never seen the 15th volume of the *Nova Acta* of the Petersburgh Academy. It may contain some papers by Lowitz.

T.

ARTICLE II.

On the Number of Inhabitants in Russia, and on the Progress of its Population, according to the Statements made by order of Government. By C. T. Herrman.*

The statement of the population of any country is of itself an object of considerable interest, because it proves the degree of comfort and happiness which the inhabitants enjoy,—makes us acquainted with the degree of power which the government is capable of exerting; but as far as Russia is concerned, this question has acquired an additional degree of interest, in consequence of the very different statements that have been published respecting her population. Schlözer states† that during his residence in Russia he represented to his superiors how surprizing it was that the total number of inhabitants was unknown, and that the discrepancies on this subject appeared incredible: and B. F. I. Hermann affirms,‡ that there is no state in Europe in which the data for determining the population are so different. The author of Essay on the Commerce of this Empire (Le Clerc) admits 14 millions of inhabitants; Voltaire, Marshal, and Williams, 18; Leveque, 19. Busching, in the first edition of his Geography, makes the number 20; in his last edition, 30; Albaum, 22; Coxe, 23; Sussmilch and Ebeling, 24; Crome, 25; Pleschtcheef, 26½; Hupel, 28; Beausoble, 30; Schlözer, in the history of his life, 33; Meusel, in the last edition of his Statistics, between 35 and 36; Storch, 36; Sablowski, in his Geography of Russia, 41, and in his Statistics, 44; and, finally, the St. Petersburgh Almanac for 1808 makes the number 42 millions. One would be tempted to suppose either that government had not occupied itself with this subject, or that it had concealed the results obtained.

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* Translated from the Memoirs of the Petersburgh Academy, vol. iii.; published in 1811.

† *Schlözer.* Histoire de sa Vie, t. i.

‡ Journal Statistique, t. i. partie 2, p. 19.

But government has been in the habit of making partial enumerations for nearly a century, and for above 60 years it has made general ones. The results of these statements constitute the object of the first part of this memoir.

As to the complaints of the incredible difference of the statements respecting the totality of the Russian population, I do not consider them so well founded as is generally believed, as will appear from the inquiry respecting the increase of the population of Russia, which constitutes the subject of the second part of this memoir.

PART I.

Result of the partial and general Enumerations made by order of Government.

Partial enumerations of the inhabitants of Russia have been made with great care ever since the year 1720. Those which embrace the greatest number of the population, and which are executed with the greatest care, are the *revisions*, the first three of which were partial, and had only a financial and military object in view. By *revision*, is meant the registering of the names of all the men who paid the direct imposts. These are divided into three classes. The first pay the capitation tax, and supply the military levies: this class consists of peasants. The second, consisting of the inhabitants of towns, merely pays the capitation tax. The third, consisting of the merchants, merely pays an impost upon capital. The statements respecting population have been divided into two compartments: the first comprehending all those that pay direct imposts; namely, the peasants, the inhabitants of towns, and the merchants: the second, all those who do not pay. But the class of peasants is properly understood when we talk of revisions. Besides these partial enumerations, which regard the great body of the nation, each department is in the habit of taking lists of those persons that depend upon it. The clergy are registered in the Holy Synod. The nobility have their registers kept with great exactness in the Russian and German governments, and less correctly in provinces that formerly belonged to Poland and Turkey. The military are enrolled by the Minister at War, &c. Russia possesses likewise general enumerations, first in consequence of the extent given to the 4th and 5th revisions in 1781 and 1796, and afterwards by annual enumerations ordered by the ukase of the 17th January, 1800, and repeated on the 8th of September, 1802.

We cannot say that all these enumerations have been worse done in Russia than elsewhere. On the contrary, they were made with great accuracy, as far as the inhabitants are concerned who pay direct imposts; and this class is much more extensive in Russia than in those countries where the class of free citizens is more numerous, or where the imposts are chiefly indirect, or the military service voluntary. If there were a point of union, a Board of Statistics, for example, for the revision of these enumerations, we might obtain very exact results, especially now that the nation has been accustomed to them for almost a century. The most defective part of these statements is the list of those who do not pay direct imposts, do not belong to any corporation, and are not in service. This class is pretty considerable, since it includes more than a million of inhabitants. The list of the women is also defective; for, as far as I have been able to learn, they are always marked in too small a number. Those people also who live by hunting, and the Nomades, are imperfectly registered, because their wandering mode of life presents almost insurmountable obstacles to exactness and arrangement.

It is from these elements, certainly very different in their nature, that the total of the population of Russia has been made out Luckily, by far the greatest part of the whole has a very great degree of probability. It is easy to see that the total, resulting from the general enumerations made as above described, must be always below the truth.

The most remarkable partial enumerations are the first three revisions, and the sum total of peasants, annually laid by the governors before the Minister of the Interior.

The difficulty of levying the capitation tax, and of fixing the military enrolments, in consequence of the perpetual change of the population, induced Peter the Great to mark definitely the number of individuals subject to that impost, and to military duty, by making one single registration once in twenty years of all the males liable to these impositions. The total number of individuals registered continues invariably the same till a new register is drawn up, and this new register is called a revision; it being conceived that the births make up for the deaths. It is obvious from this that the population cannot be exactly known from the numbers given in the *revisions*, though these numbers always furnish the most probable base. By this method, which is unique in its kind, the genius of Peter the Great set government at its ease with respect to the quantity of impost and of military levies, and left to the different corporations subjected to direct imposts the care of fulfilling their obligations to the state in the way that they think the most convenient; and this trust is discharged in a most exemplary manner, especially in the class of peasants. The government is sure for twenty years of its revenue, and of the number of individuals on whom it can depend for military service.

The first three *revisions*, which were partial enumerations, were executed during the years 1720—1723, 1741—1743, 1761—1763. The years of revision usually quoted are 1722, 1742, 1762.

The first revision, ordered in the year 1721, and finished in the year 1723, gave the whole number of revisionaries as 5,794,928. This is the well-known number quoted in all the statistical accounts of Russia; but according to a statement of the population sent to Voltaire for his History of Peter the Great, and which is found in the first folio volume of the MSS sent to him, and which returned with his library to the Hermitage of St. Petersburgh, there were only at the first revision 5,401,083 revisionaries who paid their capitation, and were liable to be called into military service; and 34,971 not liable; making a whole of 5,436,054 revisionaries. A difference of 358,874 males is too considerable to be ascribed either to an error in the calculation, or in taking down the numbers ordered for a financial and military purpose by Peter the Great. I cannot account for the difference in a satisfactory manner: for even supposing that the statement sent to Voltaire did not include the inhabitants of towns and the merchants, which might very well be the case, as they are not revisionaries, still the number 358,874 appears to me too great for the state of industry at that time; because at present, when the population is three times as great, and the national wealth prodigiously increased, the number of inhabitants of towns, and the number of merchants, do not exceed 650,000 males. I am somewhat uncertain, therefore, about the true result of the first revision.

The second revision, which was ordered in 1741, and terminated in 1743, is likewise attended with some uncertainty. There are two or three tabular statements of the result: one gives 6,646,890 for the number of males comprehended under the revision; another gives 6,677,167. Georgi makes the number only 6,643,335. The difference of 30,777 between the two statements is much smaller than the discrepancy which exists with regard to the first revision, and agrees pretty well with the number of revisionaries which, at the first revision, did not pay the capitation tax. I have no data for accounting for the difference.

The third revision, of 1762, is the only one which gives only one result. The total number of revisionaries is marked at 7,363,348.

This would be the proper place to speak of the annual general enumerations of the peasants made since the year 1800: but not to interrupt the chronological order, I pass to the fourth revision, begun in 1781, and terminated in 1783, with which the general enumerations in Russia commence.

The object of this general *revision* was to relieve the people, who necessarily suffer, at length, in consequence of the inequality introduced into the division of the imposts, and of the civil and military services, by the loss of those who die, for whom it is always necessary to pay, while those persons who are born subsequent to the preceding revision pay nothing. This object is even announced in the ukase of the 30th June, 1794, for the fifth revision. The principles of this general enumeration were, that nobody might be exempt from the revision, and that every one should be registered according to age, sex, and station in life. See the Manifesto of Nov. 22, 1781, sect. 1 and 12.

The method followed in this enumeration is almost that of *Sonnenfels.* Printed formulas were sold at a low price, which the magistrates distributed in the towns to the overseers, directors, old men, farmers; but nobody was obliged to purchase this formula, and the report might be written on a bit of ordinary paper. The third section is particularly remarkable, where it is said, "it is necessary to note down in the reports for the country the number of inhabitants and of their domestics, according to their age and sex, adding the number of new establishments made since the last revision, and noticing whence the peasants have been brought for the new habitations, noticing likewise if any town has been ruined by any accident, and to what place the inhabitants have retired." In this order we perceive a superior object to that of the finances and military levies, an object truly economical. A knowledge thus accurate of all the changes which have taken place in the country must constitute the best foundation of a political legislator. There can be no doubt that answers have been given to these questions, put with so much wisdom, but I am uninformed as to the result. A general table would have made us acquainted with the progress of agriculture and of industry in the country. The data respecting the results of this fourth revision are different from each other, and imperfect. Georgi says there were 12,527,699 males subject to the direct impost, and 310,830 exempt from it. Storch includes the two sums of Georgi when he states the number at 12,838,529; but Hermann gives in all probability the true sum total when he makes it 13,176,411 males. The difference between the two statements is 340,882. I conceive the first number to be the amount of the revisionaries; the second, that of the inhabitants of towns and merchants; and the difference, the number of nobles and clergy. Such differences always exist in the data of statistical writers, and even in official reports. All these sums may be accurate. The difference probably arises merely from taking a particular sum for the total. The number of women is totally wanting, and on that account the amount of the fourth revision is in part unknown.

The fifth revision, ordered in 1794, and terminated in 1796, included likewise all the inhabitants of Russia, with the usual exceptions; that is to say, excluding the two capitals, the army, the Nomades, and the people who support themselves by hunting. It was executed with much exactness during the reign of Paul I. Hence it is one of the most remarkable enumerations made in Russia. I have been lucky enough to obtain the results of this enumeration, though they have not been made public. I have received two statements of the population from this fifth revision, the first from the Senate, the second from the Ministers of the Finances. The first served for farming out the duties on spirits in 1803, and contains only the males. Its exactness may be depended on. It is entitled, Number of Males according to the Fifth Revision. The number is 17,815,370. The second statement is very circumstantial, and includes both sexes. It gives 17,800,536 males. The small difference between these two statements is owing to the existence of some blanks in the last table, which they could not venture to fill up, because the official documents had not arrived. Hence the first number is the correct one, and the second serves only to confirm it.

The number of females stated in the last table is 16,223,229. This number appears to me too small, and probably proceeds from some inaccuracy in taking down the numbers of the females.

According to the preceding statement, the final result of the 5th revision is—

Males	17,815,370
Females	16,223,229
Inhabitants of Russia	34,038,599

with the exceptions above-mentioned.

We come now to the annual enumerations. The ukase of the 17th January, 1800, orders the civil governors to make annually exact statements of the quantity of grain sown and reaped, and likewise of the state of the population, without excepting any person. This decree was repealed on the 8th of December, 1802, by the Senate. The civil governors are bound to give as much exactness as possible to the reports of the quantity of grain sown and reaped in their respective governments, and likewise of all the inhabitants, without exception, who reside in them.

Since that period there have been general enumerations repeated every year all over Russia. The following are the results of the first five years as I have received them from the Minister of the Interior.

The sum total of all the inhabitants of Russia of both sexes was—

In 1800	33,159,860
1801	34,043,357
1802	34,893,828
1803	35,134,177
1804	36,043,483

I have received from the same quarter the following data for determining the number of individuals not comprehended in these statements.

1. The inhabitants domiciliated in Petersburgh, not reckoning the military (estimated at between 30 and 40,000), amount to 170,000. This number may be true with respect to the inhabitants

domiciliated, but it is greatly below the usual population of the place. This happens generally with respect to all the Russian towns. In the year 1789, the number of inhabitants in Petersburgh amounted to 217,000, without reckoning the military: in 1803, to 244,000: and in 1810, to nearly 300,000.

2. The inhabitants of Moscow are reckoned at 300,000. The number of domiciliated in that place amounts to about 240,000, but in winter it augments to about 400.000.

3. The military are estimated in round numbers at 400,000: but this is much below the truth. From the statement drawn up in 1805, it appears that the guards, the cavalry, and the infantry, alone form a body of 362,223; the artillery and the *genie*,* 45,000; and the garrisons, at least 111,420; making together a sum total of 518,643. If to this we add at least 100,000 irregular troops, it is obvious that the land forces of the empire surpass 600,000.

4. The Nomades are estimated at 1,500,000 individuals of both sexes.

It is easy to see that these four sums are but approximations. I admit in round numbers, 1. Inhabitants of Petersburgh, 240,000:—2. Inhabitants of Moscow, at a medium, 320,000:—3. Land forces, 600.000; their wives and children, 300,000:—4. The Nomades, 1,500,000:—Making a total of 2,960,000: which added to the preceding statement of the population in 1804 makes a sum total of 39,003,483.

But even this sum total is below the truth, according to the statements made to me by the Minister of the Interior. The following are the observations to which I allude. "A comparison of the reports made by the civil governors with the statements in the fifth revision, has shown that several governors have merely copied the ancient statements, giving the classes subjected to direct imposts, without noticing those who are exempt from them. In several governments the number given differs very little from the number of the first class marked in the revision, even in those governments where there are a numerous nobility and populous cities. At Novogorod, the difference is only 700 individuals of both sexes; at Smolensk, 16,000; at Plescow, 15,000; at Kalouga, 7,000; at Resan, 9,000; at Kasan, 3,000. At Vaetha the annual enumeration is below the fifth revision. Considering the defects that still exist in the annual enumerations, we may very well make an addition of 20,000 individuals for each government, which would make a million for 50 governments, and would raise the population to 40 millions. Finally, the surplus of births above deaths amounts annually to 500,000, which in the ten years between the last revision, in 1796 and 1806, amounts to 5 millions. If we allow a fourth of the births to reach the age of 18 or 20, we may estimate the real progress of population at 1,250,000 individuals."

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* This is the original word: I have not translated it, because I am not sure what sort of troops it alludes to.—T.

From these elements we should have for the population of Russia in 1806—

1. The sum total of the enumerations of 1804	39,003,483
2. Compensation for imperfections	1,000,000
3. Progress of population during 10 years	1,250,000
Total	41,253,483

This is the number of inhabitants in Russia known by the enumeration, and rectified by probable estimates.

The surplus of 3,000,000 between the enumeration of 1806 and 1800 is not the effect of the rapid increase of population, but the care taken to bring the annual enumerations to a state of greater perfection. I conceive that, during the succeeding five years there will be a new surplus, but probably not amounting to more than one-half of the preceding. I suppose, therefore, that the annual enumeration may in 1811 amout to 42½ millions, or even 43 millions. After this they will remain stationary for a long time at the same sum; for if we examine impartially the state of the agriculture, manufactures, and commerce, of Russia, it appears to me that these different branches of industry, on which the progress of population depends, have attained that degree of perfection which the present state of the wealth of the empire permits. Peace, and fortunate and unexpected occurrences, may indeed carry them to a higher degree of perfection; but such suppositions are beyond the province of statistics.

I add, as a very interesting document, the total number of revisionaries, in the strict sense of the word, or of peasants as stated in the lists. This number was—

In 1796	15,718,083 males.
1800	15,707,781
1801	15,747,379
1802	15,895,608
1803	15,824,287
1804	15,806,778

Respecting this statement I received the following observation. "The difference in these sums arises from the migrations of peasants from one government to another; for it happens when these changes take place that one chamber of finance strikes off the peasants that have left the government before the other can include the new comers, or that both include the same individuals in their registers, or both omit them entirely."

I have not hesitated to state the uncertainty that still exists with respect to the enumerations made by Government. I think this part of the administration might be brought to a greater degree of perfection: but we must never expect mathematical accuracy, because such enumerations are not susceptible of it; neither must we suppose that these imperfections are peculiar to Russia.

When the Committee of Division of the Constituent Assembly made an enumeration in 1791, the result was 28,896,000 individuals. Some years after, by a second enumeration, this sum was reduced to 26,363,000.*

In Hungary a first enumeration, made in 1785, gave 7,008,574 individuals; a second, in 1786, 7,044,462; and a third, in 1787, gave 7,116,780: and after all the sacrifices that Hungary has made within these few years, the enumeration of 1810 gave 7,398,104: a proof of the imperfection of the preceding enumerations.†

Malthus gives to England and Wales, in 1677, five millions of inhabitants; Petty, in 1682, makes them 7,400,000; Davenant, in 1692, makes them between seven and eight millions; King, in 1699, admits only five millions; and Derham considers this number as the most correct; Decker, in 1742, supposes 7,200,000; Mitchel, 5,700,000; Brakenridge, 5,340,000; and Price, 5,500,000.

A philosopher, who was himself employed in Prussia to determine the state of the population, makes the following avowal.‡ "Those statements which bear the name of Historical Tables of Prussia are drawn up by the lowest class of the commissioners of the police, who consider this duty as a troublesome and useless burden. The commissioner seldom visits all the houses; he commonly corrects an old register, from his local knowledge: even if he does visit every house, he never thinks of verifying the statements which he receives by having recourse to the statements of the neighbours; and if the master of the house be from home, he fills up the blanks by guess, to save himself the trouble of paying a second visit. All these difficulties, which exist even in the cities, are augmented in the country. The formulas being too complicated for the old people of the village, they naturally arrange all the men under one head, and all the women under another, without distinguishing the age, the condition, the travellers, &c. Hence the statements of the population of the country are always above the truth. I have myself pointed out several inaccuracies; and as it frequently happens that another department reckons the very same individuals at the same time for another object, it is usual to find mistakes of some hundreds in a total which does not exceed a thousand."

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* Herbin, Statistique Generale et Particuliere de la France, t. i. Art. *Population.*

† Schwartner Statistique de l'Hongrie, 1798, p. 71.

‡ Allgemeine Literatur-Zeitung, 1805, No. 265.

(To be continued.)

ARTICLE III.

Remarks on the Essay of Dr. Berzelius on the Cause of Chemical Proportions. By John Dalton.

(Read before the Manchester Society, Dec. 24, 1813.)

It may perhaps seem premature to animadvert on an essay before the whole of it has been published; but as Dr. Berzelius has stated certain objections to the atomic theory of chemistry in that part of his essay published iu the *Annals of Philosophy* for this month, which, if left unanswered, may be thought by some to present insuperable difficulties, I have judged it expedient to make a few remarks on the subject immediately, by way of obviation. Engaged as I have been for some years past, and still continue to be, in a labyrinth of chemical investigation, it may well be imagined I cannot find much time for controversy; yet the scruples of one who has so eminently distinguished himself as a fellow labourer in the same field of science, and whose views and opinions in a great measure approximate to my own, are certainly entitled to consideration. Whatever our theoretical speculations may be, they are of little avail unless supported by facts; aud, notwithstanding the modern improvements in the practice of chemistry, no theory of it can advance far without meeting with the difficulties which too often arise from inaccurate observation. I hope to prove to the satisfaction of Dr. Berzelius that some of the difficulties he finds in pursuing the atomic theory are of this kind, and that the rest are only imaginary.

The first division of Dr. Berzelius' essay, on the relation between Berthollet's theory of affinities and the laws of chemical proportion, contains an admirable exposition of those facts which Berthollet brought forward in so conspicuous a point of view in his chemical theory, and which his zealous followers have magnified in a still greater degree. A better explanation could, I think, be scarcely given in fewer words.

In the second division on the cause of chemical proportions, Dr. Berzelius, after ascribing to me the principal share in announcing and developing the corpuscular or atomic theory, proceeds to give an explanation of what *he* conceives it to be. His ideas on this head are somewhat at variance with mine; and this is one point on which I wish to be clearly understood, and shall endeavour in what follows to enable the reader to discriminate betwixt us.

Dr. B. seems to hold it necessary that all atoms should be of *the same size*. This, he thinks, is required in order to form bodies into regular figures. Now this is no part of my doctrine. I do maintain that all the atoms of any homogeneous body, A, are of the same *size* as well as weight; and that all the atoms of B are of the same size and weight; but I see no sufficient reason for concluding that the atoms of A are of the same *size* as those of B. The probability is rather, I think, that the atoms are of *unequal sizes*; and the size may be in direct proportion to the weight, or otherwise. There can, in fact, be only three suppositions on all this subject:—1. That the sizes are all the same. 2. That the sizes are as the weights. 3. That the sizes are unequal, but not as the weights. My system is not restricted to any of these suppositions; but if any one can show that the regular organization of bodies is inconsistent with one or other of these suppositions, it must, of course, be rejected. Till that is done, one is about as plausible as another.

It is rather amusing to me to observe the different manners in which a cursory view of the atomic system strikes different persons. Dr. Thomson was the first who, from some hints I gave him, published an outline of the system in the third edition of his chemistry. He used the phrase *density of the atoms* indifferently for *weight of the atoms*, thereby implying that all atoms are of *the same size*, and differ only in *density*; but he has since very properly discontinued the use of the phrase. This also appears to be the notion of Berzelius. On the other hand, Dr. Bostock seems to think (see Nich. Jour. vol. xxviii. p. 292) that the *sizes* of atoms must be in direct proportion to their *weights*.

Dr. Berzelius thinks it necessary that when an atom of A combines with an atom of B, it must touch it. We shall agree in this mode of expressing the fact; but our ideas may differ materially with regard to the signification. The contiguous atoms of all elastic fluids touch each other by means of thin atmospheres of heat: I neither know nor admit of any other sort of contact. The solid impenetrable matter, if there be such, constitutes the centre of the atom, never comes into contact with that of any other, as far as is known; because it appears to be impossible to deprive bodies of their heat. The atoms of bodies may, therefore, co-exist at various distances; in the solid and liquid forms they are comparatively near, and in the elastic form distant; but in all the forms they are subject to variation, in this respect, from temperature and pressure. It is probable that the atoms of oxygen gas might be condensed into a volume, so that their distance should not exceed that of oxygen and hydrogen in an atom of steam, and yet not unite chemically so as to change their form. Hence it should seem that the notion of particles *touching* each other is not a sufficient criterion of chemical union. In elastic fluids chemical union is best conceived, I think, from the circumstance of two or more atoms of A and B uniting so as to form a common centre of repulsion.

With regard to the *figure* of atoms, Dr. Berzelius observes, that a compound atom cannot be considered as spherical, but that an elementary atom may be taken as such. Here, again, we should understand one another, whether *solid corpuscle* is meant, or *solid corpuscle united to an atmosphere of heat*, when we speak of an atom. If the former, then it is clear that compound atoms cannot be spherical; nor do I see any reason sufficient for supposing all simple atoms to be so; those of hydrogen may be spherical, perhaps; those of oxygen may be regular tetrahedrons; those of azote may be cylinders of equal diameter and altitude; &c. &c. But if we understand atom in the latter sense, then not only the elementary atoms, but most of the compound atoms, are probably spherical, spheroidical, or some figure approaching to that of a sphere. Of all compound atoms, that consisting of 3 elementary atoms is probably most remote from a sphere; but when the compound one contains 5 or more simple ones, the figure must, I should suppose, be virtually a sphere.

What Dr. B. says of the *electric polarity* of atoms (vol. ii. p. 447) makes no necessary part of the atomic theory such as I maintain. Neither does the conclusion that 2 atoms of A cannot combine with 2 of B, 2 of A with 3 of B, &c. Such combinations, I apprehend, rarely exist; but I see no reason, either from theory or experience, for rejecting them. It may be said that such compound atoms are capable of division: true. But the parts may instantly unite again by virtue of an affinity; and hence they cannot, perhaps, be exhibited in a divided state. Olefiant gas, for instance, is known to consist of carbon and hydrogen united in the ratio of 1 atom to 1: but there is nothing that I know of to prevent their uniting 2 atoms of carbon to 2 of hydrogen, and the 4 atoms to be placed in the form of a rhombus, those of hydrogen being at the extremities of the longest diameter. Nitrous acid, too, may be adduced as an instance of 2 atoms of azote and 3 of oxygen. And even nitric acid seems most frequently to be found in composition as if constituted of 2 atoms of azote and 4 of oxygen united to 1 of base. However unlikely this may be, I see no absurdity in supposing that if 2 atoms of nitric acid, such as I have delineated in my Chemistry, were contiguous, they might coalesce by affinity, and refuse to take a third atom or compound of the like kind. Hence I disclaim the axiom that every compound atom must have a single atom for its nucleus or centre.

So much for the differences in our conceptions of the principles of the atomic theory. We come next to the difficulties to which Dr. Berzelius apprehends it is liable.

"The first of these difficulties is the circumstance that there are combustible bodies, *iron*, for example, which unite only with two doses of oxygen, the second of which is only 1½ times greater than the first." Here I perfectly agree with Dr. Berzelius, both as to the existence of the difficulty, and as to the solution of it which he has given. When the oxygen in the (supposed) protoxide of any metal is to that in the deutoxide as 1 to 1½, that is, as 2 to 3, it is to be presumed that the real first oxide is not known, and that those two which are known are the second and third oxides. But this is not the only solution which such cases admit of, as will appear presently. My ideas on the oxides of iron were settled from some experiments I made in 1807, compared with the experiments of others then published. I concluded that 100 iron combine with 28 oxygen by solution in sulphuric acid, forming what is called the black oxide; and with 42 oxygen by heat, &c. forming the red oxide. Hence 50 iron combine with 14 oxygen and with 21, and the combination of 50 iron with 7 oxygen to form the protoxide is unknown. It was in conformity with this reasoning that I published in the second part of my Chemistry, in 1810, the atom of iron to weigh 50. The details were not given, because I was not then describing the oxides. The facts are not at all at variance with the atomic theory; but it appears a strange circumstance that the first oxide cannot, by any means we are yet acquainted with, be obtained, whilst the second and third are readily. The circumstance, however, is not without parallels. We well know that sulphur takes 2 portions of oxygen to form sulphurous acid, and 3 to form sulphuric; but we scarcely recognise a compound of sulphur with 1 portion of oxygen. Again, carbonic acid gas has been known time immemorial; but carbonic oxide gas has been known only about twelve years: yet it is pretty evident that the latter is, theoretically, the more simple combination of the two.

The second difficulty which Dr. Berzelius states is so obscurely expressed that it requires an acute atomist to perceive the force of it. This may, perhaps, be partly owing to the translation, and to an error of the press, which last, however, is pretty readily corrected. He has discovered a law (which, for the sake of argument, I shall take for granted to be true,) "that when two oxides combine they always unite in such proportions that each contains either an equal quantity of oxygen, or the one contains a quantity which is a multiple by a whole number of the oxygen in the other." This law, though in itself conformable to the corpuscular theory, admits, (he says) on the one side, of combinations inconsistent with that theory; and, on the other side, it excludes combinations perfectly conformable with that theory. To illustrate these positions, he gives several examples: I shall take the first two. Let O be oxygen, A and B two combustible bodies; then A + 3 O may combine with B + $1\frac{1}{2}$ O, because $1\frac{1}{2} \times 2 = 3$; and (he asserts) that such combinations exist, though according to the corpuscular theory they appear absurd. Now I think it must be obvious that this *second* difficulty is the same as the first, and admits of the same explanation which Berzelius has given; namely, that the body, B, in such case has in reality 3 atoms of oxygen for 1 of metal, the quantity of which oxygen we chuse to express by $1\frac{1}{2}$. And the union in question is 1 atom of the third oxide of B with 2 atoms of the third oxide of A, a combination perfectly consistent with the atomic theory, as well as with the law and the example just exhibited to view. The other example is, that the law does not admit A + 3 O to unite with B + 2 O, though such combination be conformable to the theory of atoms. In reply to this, I may observe, that it is not the peculiar business of the atomic theory to explain why A + 3 O do not unite with B + 2 O, any more than to show why all the metallic oxides do not mutually combine with each other; for it may be said there is nothing apparent in the atomic theory to prevent such combinations.

After having the atomic principles in contemplation for ten years, I find myself still at a loss, occasionally, to discriminate between the combinations which contain 2 atoms of a given body from those which contain only 1 atom. Hence an atom that weighs 50 may be sometimes put down as weighing 100. It is owing to the difficulty on this head, I apprehend, that Berzelius considers the atom of lead at twice the weight I do; in consequence, he makes the yellow oxide of lead to consist of 1 metal and 2 oxygen. As for the red oxide of lead, I consider it, after Proust, as being probably made up of the yellow and brown oxides in combination.

One example adduced as incompatible with the atomic theory appears to me peculiarly unfortunate. It is an oxide of iron containing 37·8 oxygen upon 100 iron, discovered by Gay-Lussac. Now this may be accounted to be a compound of 2 atoms of the red oxide and 1 of the black; for such a compound must contain 37·3 oxygen upon 100 of iron, which agrees more nearly with the experiment than we have any right to expect in such case.

The third and last difficulty which Dr. Berzelius has brought forward as militating against the atomic theory is derived from the analysis of what he calls *organic atoms*; that is, atoms composed of more than two elementary substances. The atom of oxalic acid is adduced as an instance.

It would be a singularly curious circumstance, and well worth recording in the annals of chemistry, if the composition of the oxalic acid itself should bid defiance to the atomic theory, or to that of definite proportions, whilst the compounds formed with it were originally produced by Dr. Wollaston and Berard as exhibiting the most striking illustration of the doctrine. I was indeed surprised to see the results of such an analysis of oxalic acid published by Berzelius, to whose accuracy in general I can subscribe; but still more so to have it afterwards referred to as militating against my doctrine. He concludes its constitution must be 1 atom of hydrogen, 27 of carbon, and 18 of oxygen; that is, 1 atom of hydrogen with 45 other atoms. Were it a matter of necessity, an atomist might conceive 1 atom of hydrogen surrounded by 9 of carbon, and the compound globule to have 18 atoms of carbonic oxide adhering to it. But this would be an atom truly formidable, in every sense of the word, as the least friction must be supposed capable of producing a violent explosion of such a mass of elasticity. I cannot, however, doubt that Dr. Berzelius, having resumed the consideration, will very soon discover and acknowledge that his analysis is incorrect. In the mean time, I shall give my reasons for believing it to be so.

Dr. B. informs us in the Ann. de Chim. (tom. 81, p. 300) that 10 grains of oxalate of lead yielded by heat 7·42 of yellow oxide. Hence he infers the constitution of oxalate of lead to be 25·2 acid and 74·8 oxide per cent. It is from the analysis of this compound that he derives his knowledge of the elements of the acid. My analysis of oxalate of lead, recently repeated, gives me 29 acid and 71 yellow oxide. Here then, in the offset, is a very material difference betwixt us as to facts: any chemist, however, is competent to satisfy himself on this head without appealing to authorities. Let a solution of acetate of lead be treated with oxalic acid, or any soluble oxalate, and the oxalate of lead will be immediately thrown down. Let it be carefully washed, dried, pulverized, and again dried in a temperature of 100°. If, then, 137 parts of this be put into a platina or iron spoon, and be very gradually heated to a low red to prevent loss by decrepitation, there will remain 97 parts of pure yellow oxide; giving the constitution of the oxalate as under; namely,

Lead	90
Oxygen	7
Oxalic acid	40
	137

The quantity 137 is here preferred because the numbers thence resulting represent those for the respective atoms upon my system. According to the analysis of Berzelius, an atom of oxalic acid would weigh only 32·7, which is less than that of sulphuric acid; in fact, he finds $26\frac{1}{3}$ sulphuric acid in sulphate of lead, and only 25·2 of oxalic acid in the oxalate. Now it happens that all the modern chemists who have analysed the oxalates (Berzelius excepted) agree with me in making the oxalic acid heavier than the sulphuric, as may be seen from their analyses of the oxalate of lime below:—

	Lime.	Oxalic Acid.
Dr. Thomson	24	40
Gay-Lussac	24	38
Berard	24	32

N.B. The atom for sulphuric acid is 34 on this scale.

About two years ago I made a series of experiments on oxalic acid and the oxalates. I then determined (as I conceived) the constitution of the acid, assisted very materially by the masterly analysis of Gay-Lussac, with which I found my results very nearly accord, as well as by that of Dr. Thomson. The atom of oxalic acid, I apprehend, is constituted of 1 hydrogen and 2 carbonic acid; or of 1 hydrogen, 2 carbon, and 4 oxygen; the total weight being 39·8, or 40. This being reduced to 100, and compared with the modern analyses, the results will stand as under. Oxalic acid is composed of

	Per Theory.	Gay-Lussac.	Thomson.	Berzelius.
Hydrogen	2·5	2·75	4	·7
Carbon	27·1	26·56	32	35·0
Oxygen	70·4	70·69	64	64·3
	100	100	100	100

The crystallized oxalic acid consists of 1 atom of acid and 2 of water. The proportions are as under :—

	Per Theory.	Thomson.	Berard.	Berzelius.
Real oxalic acid..	71·4	77	72·7	71·25
Water	28·6	23	27·3	28·75
	100	100	100	100

It is remarkable that Berzelius determines the water in the crystals of oxalic acid, as it should seem, with great accuracy; but that when the acid is combined with lead it loses (according to him) about 14 per cent. *more* of water. Now this does not happen in the case of the other insoluble oxalates, such as that of lime; for Dr. Thomson found 77 parts out of 100 of crystallized oxalic acid to exist in the dry oxalate of lime obtained from it. This circumstance is of itself sufficient to render Berzelius' analysis of the oxalate of lead doubtful. Query: What temperature was his oxalate of lead dried in?

With respect to the *theory of volumes*, to which Berzelius seems inclined to give the preference rather than to that of atoms, it is not my intention to say much at present. I shall wait to see it more fully developed. I own I do not see how we are to remove the difficulties attending the atomic theory by substituting the term *volume* for that of atom; nor how "we can figure to ourselves a *demi-volume*, while in the theory of atoms a *demi-atom* is an absurdity." Notwithstanding this, whatever may come from the pen of Berzelius on the subject will, no doubt, be worthy the attention of the chemical world.

ARTICLE IV.

On the Porcelain Earth of Cornwall. By William Fitton, M.D. of Northampton.

(To Dr. Thomson.)

DEAR SIR,

The enclosed paper was drawn up a few years ago, at the desire of some friends, from notes which I had taken in Cornwall: it has not been in my power to render it complete; but if you think it worth inserting in your Journal, it is at your service.

I am, Dear, Sir, &c. &c.

Nov. 1813. WILLIAM FITTON.

The principal earthy substances obtained in Cornwall, that are used in the manufacture of the finer kinds of earthenware, are:

1. *Steatite,** which is found at Cape Lizard in what are described as veins† that traverse rocks of serpentine. 2. *Granite,* the felspar of which is in a disintegrated state. 3. *Porcelain earth.* The object of this paper is to describe the mode of preparing the two last-mentioned substances for exportation.

It is not improbable that *porcelain earth,* or, as it is denominated by the workmen, "*China clay,*" may be found in several parts of Cornwall; but the preparation of it for the manufacturer was, when I visited that country in 1807, confined to the parishes of St. Stephen and St. Denys, in the neighbourhood of St. Austle. There were at that time seven different works, viz.: two at *Hendra,* in the parish of St. Denys; and in that of St. Stephen, two at *Treviscoe,* and one respectively at *Gonnemarris, Goonvean,* and *Trethorsa.* These were carried on by manufacturing companies of Staffordshire and other places, for their own consumption; and I believe in one instance by a company at Plymouth, who prepared the clay and sold it to manufacturers who had not works of their own.

The country around the works is a wild and barren moor; the ground being in general uninclosed, and of very little value for the purposes of agriculture. The rock which forms its basis is granite, consisting of a large proportion of felspar in a disintegrated state, with little mica and quartz, and some specks of a greenish substance, probably steatite.

To each of the works for the preparation of the porcelain earth there is annexed a "quarry," where the granite itself is extracted, in a solid form, for the use of the manufacturer. It is detached from the rock by means of blasting and wedges, and then broken into pieces of a convenient form for exportation, the hardest of which are preferred, on account of their more easy carriage. These pieces, as well as the prepared "China clay," are conveyed in carts to St. Austle and to Charlestown, in the neighbourhood of that place; from whence the greater part, if not the whole, is shipped for Plymouth, and thence to the potteries, where the *granite* is subsequently prepared for the manufacturer by grinding and washing.

The "clay pits," as the places are called where the *porcelain earth* is dug, are all situated in "bottoms," in the course of vallies or ravines which traverse the surface of the country; the accumulation of this earth appearing to have arisen from the detritus of the granite brought down by water from the more elevated ground. The "clay," as might have been expected from its apparent mode of deposition, is found at various distances from the surface; in some places close to it, in others several feet below. Immediately beneath the vegetable soil there is commonly a bed of lighter coloured matter: and under this, bounded at top by an irregular waving outline, the china clay occurs.

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* The names of minerals employed in this paper are those of Mr. Jameson's System of Mineralogy.

† Dr. Thomson, however, supposes the steatite at the Lizard to be "merely a portion of the serpentine itself, altered by the action of water, or some other cause."—*Annals of Philosophy,* vol. ii. p. 251.

The depth to which the clay itself extends is also various. In one of the works that I examined, the bottom of the pit in which it was dug was eight or nine feet from the surface; and the clay "held down," as the workmen expressed it, for about ten feet more. In another work, situated rather lower, in the course of a stream, the thickness of the "overburden," or incumbent soil and earth, was about nine feet. At *Trethorsa*, one of the principal works, the depth of the "clay" varied from one to eighteen feet.

The clay as it is found has somewhat the appearance and colour of mortar newly made: there are dispersed through it many angular fragments of quartz; a proof, I suppose, that it has not been conveyed from any considerable distance; and throughout the mass there appear large irregular patches and stripes of a reddish brown colour, of which also some small veins occur. This coloured part, called by the workmen "weed," is, in digging, carefully separated from the rest and thrown aside.

The process by which the clay is prepared is the *elutriation* of chemists, performed on a very large scale; the detail of the various steps as follows:—

The "overburden" being removed to a considerable extent, the clay itself is dug progressively in steps, each four or five feet deep, the vertical faces of which are cut down with pickaxes and shovels, and the whiter parts conveyed in wheelbarrows to be "*washed.*" At some of the works the clay is carefully mixed, in one large heap, before the washing; but in others this mixture is dispensed with, and it is removed directly from the pit to smaller heaps, on which a stream of water is allowed to pour, while the mass is frequently turned and supplied by a man or boy. The water in passing through the heap becomes charged with particles of clay, and is conveyed by wooden spouts to what are called the "*pits*" and "*ponds*," leaving the coarser parts behind.

These pits and ponds are merely rectangular excavations dug from the surface, and rendered water-tight by a floor and walls of cut granite, bedded in mortar made with lime from Aberthaw,* which has the property of forming a strong cement under water. The *pits* are in general about five or six feet by four, and about four feet in depth; the *ponds*, about twenty feet long by twelve in width, and four or five feet deep. At the middle of one side of each pond there is let into the wall a vertical board, pierced with two rows of holes placed alternately, and furnished with plugs, for the purpose of letting off the water gradually: and on the outside of the pond there is a small excavation lined with stone, with steps to enable a workman to descend and adjust the plugs, and an opening at the bottom, through which the water let off is conveyed to a drain underground. The *pits* also, when it is intended to preserve their contents, are furnished with a similar apparatus.

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* A village on the coast of Glamorganshire.

The water running from the heaps of clay is first received in a *pit*, which it is allowed to fill: the coarsest of the suspended particles subside, and the lighter and finer are conducted from the surface in the overflowing water by channels, or wooden spouts, to other contiguous pits of nearly the same dimensions: in these it deposites still further the coarser part of its contents, and overflowing carries off only the finest particles of clay.

In the bottom of the first pit there is an opening, with a trap or valve, through which the coarse parts that have accumulated are allowed to run off at the end of each day's work. The deposit of the *second* pits is collected from time to time, by gradually letting off the water from above it, for the purpose of being dried separately, and sent to the potteries. It bears the name of "*mica*," and appears, in fact, to consist principally of that mineral. There is, however, in this part of the process some variation, depending on the object and judgment of the manager. In some of the works the "mica" is not preserved; and in some there are *three* pits, through which the water passes before it arrives at the ponds, the deposit of one or more of them being preserved or rejected according to circumstances.

The water which has come from the pits being received in the *ponds* is allowed to extend itself, and gradually to deposit its contents. As the mass of clay increases at the bottom, the openings in the boards at the sides are successively stopped with plugs, which prevent the escape of any but the clearest water; and thus the accumulation continues until the pond is full.

The contents of the ponds, when they are filled, are transferred from them in hand-barrows to what are called "*pans*," which are shallow excavations adjacent to the ponds, and like them lined with granite. They are generally about forty feet in length by twelve in width, and about fourteen inches deep; their extent and number being proportioned to the dimensions of the ponds. The clay, now in the state of a thick mud, is distributed uniformly over the bottom of the pans to the depth of from ten to fourteen inches, with a wooden instrument like that in common use for scraping roads; and it remains to dry for a length of time, which varies from four months to eight, according to the season and the weather.* What has accumulated during the summer months, being put into the pans in September, is generally found to be firm and nearly dry about the following April or May. The depth of the mass in this state varies with the height to which the pans have been filled, and the thickness of the clay when introduced. It is now cut with large knives into blocks resembling bricks, of the thickness of the mass in one direction, and varying in their other dimensions: these bricks are transferred to the shelves of a drying-house, or shed, which are formed of wooden bars freely admitting the passage of the air between them; and when quite dry, the pieces are scraped perfectly clean with an iron instrument, and the coarser parts, containing fragments of quartz and other impurities, which formed the bottom of the mass, carefully removed. The pieces are then put into casks, and broken down by ramming so as to fill them completely, and thus sent to the potteries. The finished clay, when well prepared, is of a beautiful and uniform whiteness, and breaks easily between the fingers without grittiness.

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* It is not improbable that this long exposure to the air in a moist state may render the clay more fit for the purposes of the manufacturer, by promoting the decomposition of the felspar

The scrapings of the pieces dried under the shed, and the waste of the packing, are saved, and washed apart into a small pit near the packing-house, the overflow of which runs into one of the large ponds, the coarser matter being rejected.

There is some variety, at different works, in the relative extent of the parts. At *Trethorsa*, one of the largest, there were, I was informed, in 1810, three small streams at which the clay was washed; and the water from each of these passed successively through three *pits* of three feet and a half in depth, two of which were six feet square, and the third nine feet by six, the deposit of the last only being preserved and dried. All these pits were arranged together in a sort of court; and the water from the last three was distributed into nine *ponds*, each about twenty feet by twelve, and five feet deep; from whence the clay was laded into sixteen *pans*, each forty-eight feet by twelve, and fourteen inches in depth. In another work: there was one pit the deposit of which was rejected, and two to receive its overflow, the sediment of which, or "mica," was dried apart, the dimensions of all three being five feet by four, and four feet in depth: three smaller pans for drying the "mica:" and four ponds, twenty feet by twelve, and four feet deep, with four large pans in which the "clay" was dried.

The quantity of clay prepared annually at *Trethorsa* was in 1810 supposed to be about 300 tons. There were then employed at that work altogether thirteen persons: eight men in removing the overburden and raising the clay, which is paid for by the cubic fathom; three men or boys in washing; and two in attending to the ponds and pans, and in packing,—the occupations of the last five varying as the different stages of the process required their attendance.

The only buildings attached to the works are, the shed for drying the clay, already mentioned, which is formed of timber, and open on three sides; and one that includes an office for the overseer, and a store-room for casks, &c. in which also the clay is packed.

The water that supplies the works is derived from the streams near which they are placed, and is carried off by subterraneous channels, which in some instances lead to shafts communicating with mine-adits in the rock beneath.

ARTICLE V.

On Sulphuret of Carbon. By Dr. J. Berzelius and Dr. Marcet. Abridged from the Philosophical Transactions for 1813. To which are added several additional facts communicated by Professor Berzelius.*

ALCOHOL of sulphur was first noticed by Lampadius in 1796, and considered by him as a compound of sulphur and hydrogen. Clement and Desormes who obtained it by passing sulphur through red-hot charcoal, endeavoured to prove that it was a compound of sulphur and carbon. Berthollet considered it as a triple compound of carbon, sulphur, and hydrogen; and Berthollet, junior, endeavoured to prove the accuracy of the original opinion of Lampadius. The question has been at last solved by the experiment of Berzelius and Marcet; while a similar result was obtained at the same time in Paris by Thenard and Vauquelin.

It may be obtained by volatilizing sulphur slowly through red-hot charcoal, and condensing in water the oily liquid thus formed. This liquid is at first of a yellow colour, but by distilling it a second time in a heat not exceeding 110° it is obtained in a state of purity.

It is transparent and colourless, like water. It has an acrid, pungent, and somewhat aromatic taste, and a nauseous smell, quite different from that of sulphureted hydrogen. Its specific gravity is 1·272, and its refractive power 1·645. Its expansive force, at the temperature of 53·5°, is equal to a column of mercury 7·36 inches in height. It boils briskly between the temperature of 105° and 110°. It does not congeal at the temperature of 60°. It takes fire at a temperature scarcely exceeding that at which mercury boils, burns with a blue flame, and emits copious fumes of sulphurous acid. If a long glass tube, open at both ends, is held over the flame, no moisture whatever is deposited on its inside. It dissolves readily in alcohol and ether, but is insoluble in water. It readily incorporates with either the fixed or volatile oils. It dissolves camphor very readily.

When heated in contact with potassium it is not decomposed; but if potassium be heated in the vapour of this liquid, it burns with a reddish flame, and a black film appears upon the surface. On admitting water, a greenish solution of sulphuret of potash is obtained, containing a mixture of charcoal. Neither mercury nor the amalgams of silver and lead are altered by it. The alkalies dissolve it entirely, though very slowly. None of the acids exert any action on it, except the nitro-muriatic and chlorine, in a moist state. It combines with azotane under water, and prevents it from fulminating.

From a variety of delicate experiments, particularly from its passing in vapour through muriate of silver in fusion, without occasioning any alteration, it was demonstrated that this liquid contains no traces of hydrogen in its composition.

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* These additional facts, which are of considerable importance, were put into the hands of the Editor by Dr. Marcet.

When a mixture of the liquid in vapour and oxygen gas were detonated over mercury, there were formed sulphurous acid, carbonic acid, and carbonic oxide gas: Hence it was obvious that its constituents were carbon and sulphur. Many unsuccessful attempts were made to determine the proportion of the constituents; the method that at last succeeded was to pass the oil very slowly through red-hot peroxide of iron, and receive the gaseous products in a jar over mercury. Most of the sulphur combined with the iron, and formed a sulphuret. The gaseous products were a mixture of sulphurous and carbonic acids. The brown oxide of lead being introduced absorbed the sulphurous acid gas, and left the carbonic. The iron was dissolved in nitro-muriatic acid, the iron thrown down by ammonia, the liquid neutralized by muriatic acid was precipitated by muriate of barytes, and the sulphate of barytes obtained determined the quantity of sulphur that had united with the iron. By this method it was ascertained that alcohol of sulphur is a compound of

Sulphur	84·83	100
Carbon	15·17	17·89
	100·00	

From the experiments of Professor Berzelius it appears that the sulphuret of carbon has the property of combining with saline bases. These combinations he distinguishes by the name of *carbo-sulphurets*. The carbo-sulphuret of ammonia is a solid yellow uncrystallized substance, very deliquescent, and partially decomposed by solution in water.

Carbo-sulphuret of lime is obtained by passing the vapour of sulphuret of carbon through lime heated in a tube. It is tasteless and insoluble in water. When digested in water hydro-sulphuret of lime is obtained. Carbo-sulphurets of barytes and strontian may be formed in the same way, and possess similar properties.

When sulphuret of carbon is left exposed to the spontaneous action of nitro-muriatic acid it is gradually converted into a solid white substance, resembling camphor in appearance, and possessing many of the properties of that substance. Berzelius, by some very elaborate and ingenious experiments, ascertained that it was a combination of three distinct acids, the muriatic, the sulphurous, and the carbonic.

APPENDIX.

Some additional Remarks on certain Combinations of the Alcohol of Sulphur, or Sulphuret of Carbon. By Professor Berzelius.

1. It was observed in, the paper published in the Philosophical Transactions for 1813, page 194, that the sulphuret of carbon was

capable of being partly dissolved in a solution of caustic potash, the mixture resolving itself into a carbo-sulphuret and hydro-sulphuret of potash, and a carbonate of potash. It was stated also that *metallic carbo-sulphurets* could be obtained by precipitating such an alkaline solution of sulphuret of carbon, by metallic solutions. These trials were attended with the following results; which, though evidently modified by the hydro-sulphuret and carbonate of potash contained in the solution, did not resemble those which would have been produced either by sulphureted hydrogen, or carbonic acid, and therefore were real carbo-sulphurets produced by a double decomposition.

Muriate of Cerium.—A white, or yellowish-white, precipitate, which, after 24 hours, was converted into a carbonate and hydro-sulphuret of cerium.

Sulphate of Manganese.—A precipitate of a greenish grey; the supernatant liquor opalescent and yellowish. This liquor had the singular property of assuming, when stirred, a fine purple colour at the surface, which became stronger as the precipitate diffused itself through it. This property diminished as the carbo-sulphuret of manganese decomposed itself, and after a few hours it had entirely disappeared. The decomposed carbo-sulphuret presented only a mixture of carbonate and hydro-sulphuret of manganese, having the colour and external properties of the hydro-sulphuret.

Sulphate of Zinc.—A white precipitate; the supernatant liquor colourless.

Muriate of Red Oxide of Iron.—A precipitate somewhat darker than the oxide itself. The hydrate of the oxide, when treated with the carbo-sulphuret of potash, assumed the same colour. An excess of the alkaline carbo-sulphuret dissolved the precipitate, and the solution assumed a flea colour, of such a darkness as to deprive the liquor entirely of its transparency. A small portion of this liquor, mixed with spring water, imparted to the water a colour of red wine, which after some time became greenish, and ultimately deposited a white precipitate. The precipitated carbo-sulphuret of iron is, but with great difficulty, dissolved by concentrated muriatic acid; and it exhales, during its solution, the smell of sulphuret of carbon.

The Submuriate of Antimony, treated by the carbo-sulphuret of potash, assumed a very fine orange colour.

Muriate of Tin (intermediate oxide).—A precipitate, which was at first of a pale orange colour, but in a few moments became brown.

Nitrate of Cobalt.—A precipitate of a dark olive green, ultimately turning black.

Nitrate of Lead.—A precipitate of a fine colour of arterial blood. The supernatant liquor colourless. This precipitate, when separated from the liquor, and treated with muriatic acid, does not at first appear to dissolve; but by degrees a muriate of lead is formed, and a strong smell of sulphuret of carbon is evolved. If the carbo-sulphuret of lead be allowed to stand, it decomposes itself, and after 24 hours it forms a black mass, consisting of carbonate of lead, which is soluble with effervescence in muriatic acid, and of sulphuret of lead which is not soluble. It appears that the carbon oxidates itself at the expense of the oxide of lead, and that the reduced portion of metal combines with the sulphur. The carbo-sulphuret of lead is equally produced when pulverized oxide of lead is mixed with carbo-sulphuret of potash. If the oxide be quite pure, it assumes a fine blood-red colour. The carbo-sulphuret of lead is also formed by digesting oxide of lead with sulphuret of carbon, mixed with a few drops of water; but it decomposes itself immediately after being formed. Without the intervention of water, the oxide of lead does not appear to exert any action on the sulphuret of carbon.

Nitrate of Copper.—A dark brown precipitate, almost black; after a few hours it becomes perfectly black, and then consists of carbonate and sulphuret of copper.

Muriate of Oxydule of Mercury.—A black precipitate.

Muriate of Oxide of Mercury.—An orange coloured precipitate, which was neither altered nor dissolved by muriatic acid.

Muriate of Silver.—A red-brown precipitate, which is decomposed and turns black in less than half an hour.

There remained to be ascertained whether the sulphuret of carbon combine with the metals in their reguline state, or whether the metals, in combining with the sulphur, separate the carbon from it. Potassium was selected for this inquiry. A small glass ball, filled with sulphuret of carbon, was introduced into a retort. The capillary orifice of the ball, by which it had been filled, was hermetically closed. A small platina tray, containing some melted potassium, was also introduced into the retort, which was afterwards quickly exhausted. The ball was then broken by giving a jerk to the retort, and the sulphuret was instantly volatilized in the vacuum. Heat being now applied, the potassium inflamed, and burned with a red flame. The product was black, and not easily soluble in water. The solution was black and opaque, and deposited some very pure charcoal. This proves that the sulphuret of carbon had been decomposed by the potassium. The black solution did not contain any sulphuret of carbon, but it contained a little carbon in an unknown state of combination. The solution, in decomposing itself by exposure to the air, deposited some sulphur, which had a greyish tinge, in consequence of a little carbon being mixed with it.

2. *On the Oxidulated Sulphuret of Iron.*—This substance (which is alluded to in the paper on the sulphuret of carbon, p. 197) being not generally known, it may be proper to give a short account of it. It is obtained by mixing together in a retort some pulverized red oxide of iron and sulphur, and heating the mixture at a moderate heat, until no more sulphurous acid comes over. It is necessary to apply the heat gently, in order to prevent the formation of a metallic sulphuret. The oxidulated sulphuret of iron is pulverulent, has a

chesnut brown colour, and is readily attracted by the magnet. It dissolves with great difficulty in the acids by long digestion and concentration. During this solution no sulphureted hydrogen is disengaged, the sulphur remains undissolved, and the *oxidule* combines with the acid. The oxidulated sulphuret of iron is very inflammable; it takes fire at a temperature much inferior to a red heat; and it continues to burn till it is wholly reduced to the state of red oxide of iron. It is owing to the formation of this oxidulated sulphuret that, when we try to expel by heat, in vessels imperfectly closed, the excess of sulphur from common pyrites, the sulphuret obtained is, but with great difficulty, dissolved by acids. The *oxidule* of cerium equally combines with sulphur, and forms an oxidulated sulphuret, which is pulverulent, and of an apple-green colour. It is doubtful whether the green sulphuret of manganese obtained in a similar manner is not an oxidulated sulphuret, rather than a metallic sulphuret of manganese. It is evident that those oxidulated sulphurets bear to the metallic sulphurets the same ratio as the sulphuret of potassium to the sulphuret of potash.

3. *On the Combination of the triple Acid, or Acidum Muriaticum Sulphuroso-Carbonicum* (Phil. Trans. same paper, p. 199) *with Ammonia.*—The triple acid, when put in contact with ammoniacal gas, slowly absorbs a considerable quantity of the gas. It seems, at first, to become liquid; but soon afterwards it forms a saline mass, which contains ammonia in excess. This subsalt presents the peculiar combination of a triple salt, with three acids, but with one single base, in the same way as the fluoborates present double salts, with two acids, but with a single base.

This triple salt is not deliquescent, though it gradually absorbs from the air some water of crystallization. It has first an acrid, and then a sulphureous taste, like the sulphite of ammonia; it is easily soluble in water, and this solution precipitates carbonate of lime from lime water. If the solution be acidulated by muriatic acid, muriate of barytes does not occasion any precipitate from it, proving that it does not contain any sulphuric acid.

When the triple salt is distilled previous to its having attracted any moisture from the atmosphere, it melts, bubbles, and gives out, first, ammoniacal gas, and afterwards a mixture of sulphureous acid gas and an etherial liquid, which is extremely volatile, and has a smell very analogous to that of the prussic acid, though it did not appear to produce any Prussian blue with solution of iron. After this etherized liquid, there comes over a saline sublimate, which is a mixture of muriate and sulphate and sulphite of ammonia, containing some water of crystallization. This water seems to be formed at the expense of the ammonia and carbonic acid, the etherial liquor containing the carbon of the latter, and also the azote with a portion of the hydrogen of the former. This salt is very curious in many respects, and well deserves to be more minutely examined.

Article VI.

Account of new Properties of Light. By David Brewster, LL.D. F.R.S.E.

(To Dr. Thomson.)

DEAR SIR,

When I received the last Number of the *Annals of Philosophy*, I was surprised to find, from the statement in your note, that Malus had also discovered the fact that light was polarised by refraction. How far his discovery extends, and to what length he has pushed his experiments, it is impossible for me to ascertain till I see the paper which you have promised to publish; but I shall not consider myself as seriously anticipated, unless he has discovered the law of the phenomena, the polarising powers of bundles of glass plates, and the other singular results, which I have described at length in my paper on that subject, which is in the possession of the Royal Society of London. If Malus's memoir contains results similar or analogous to these, I must then consider the labour and anxiety which attended that series of experiments as completely lost.

In the prosecution of these inquiries, to which I devote every leisure moment that I can command, I have had the good fortune to obtain a number of new results, not inferior, either in singularity or importance, to any of those which have yet been published respecting the affections of light. As some time must elapse before the papers which I have written on these subjects can be published, I have taken the liberty of transmitting to you a hurried and imperfect abstract of the leading phenomena, and shall esteem it a particular favour if you can find room for them in your Journal. From this abstract I have excluded the results which you have already given in your history of science for 1813. The experiments have been often repeated by myself, under various modifications: some of them have been exhibited before the Royal Society of Edinburgh, and the greater part of them have been performed before some of its most distinguished members.

I. *Experiments on the Depolarisation of Light.*

1. Almost all minerals possess the property of depolarising light, or of robbing it of its polarity, and have two depolarising axes, and also two neutral axes in which no depolarisation takes place. The neutral axes of mica and topaz coincide with the diagonals of their primitive rhomboidal base. Out of 14 diamonds 7 had no neutral axes, but depolarised light in every direction; three did not depolarise it at all, and the rest produced a partial depolarisation.

2. *Caoutchouc* cut in any direction, *camphor*, *gum arabic*, *horn*, *tortoise shell*, &c. have no neutral axes, but depolarise light in every direction.

3. All soft substances that lose their lustre on cooling; and others when formed into a thin plate by heat, between two pieces of glass, depolarise light in every position. The most remarkable of these are white wax, spermaceti, adipocire from human bodies, oil of mace, acetate of lead, manna, adipocire from animal muscle, adipocire from biliary calculi, tallow, benzoic acid, oxalic acid. Of these substances oil of mace exhibits a series of surprising phenomena. In some parts of the plate it restores the complete image of the vanished candle: in other parts it restores two condensed nebulous images of the candle, separated by a small interval: and in another place it depolarises or restores *four nebulous wings*, or *sectors of light*, separated from each other by four dark and broad radial lines, at the meeting of which is the place of the vanished image. The other phenomena presented by this remarkable substance could not be rendered intelligible without the aid of a figure.

4. The phenomena exhibited by *tallow* are equally interesting. When first cooled, it does not display, like the other bodies, any optical indications of a crystallized structure; but after standing five or six days, an incipient crystallization begins to develope itself, by the depolarisation of a small quantity of nebulous light. This nebulosity increases, and about the 16th day four luminous sectors, like those of oil of mace, are imperfectly developed. This effect of time in completing the crystallization of bodies leads to new speculations respecting the structure of what has been considered as mere unorganized matter.

5. Light may be polarised and depolarised in the same body, the incident pencil being common light, and the emergent pencil depolarised light.

II. *Theory of the Depolarisation of Light.*

When I first discovered this property of crystallized bodies, I naturally referred it to a cause different from that of double refraction. The faculty of depolarising light was possessed by many substances that gave no indications of double refraction, and even by animal and vegetable products, such as horn, caoutchouc. The circumstance, however, of agate and Iceland spar having the property both of polarising and depolarising light, and the constant relation in the position of the axes which regulate these apparently opposite actions, induced me to think that the two classes of phenomena had the same origin. This opinion was afterwards confirmed by an experiment with a bundle of glass plates, by which the vanished image was depolarised by polarising it in a new plane; but in applying the principle to other phenomena, I was baffled in every attempt to generalize them. By extending, however, and varying the experiments, I have discovered the general principle to which they all belong, and have thus been led to several conclusions, which could not easily have been obtained by direct experiment. In a short notice like the present, I cannot hope to make this theory well understood, without the details which I have given in my larger paper on the subject; but some idea of it may be formed from the following brief statement.

When the polarised image of a candle is doubled, by viewing it through a prism of Iceland spar, the images vanish alternately in every quadrant of the circular motion of the prism. If the vanished image is depolarised or restored, by the interposition of a plate of topaz, none of the images will vanish during the rotatory motion of the prism. Now the topaz being a doubly refracting crystal actually produces two images of the polarised candle, and therefore upon turning the topaz round one of these images must vanish in every quadrant, though from the images not being separated this vanishing is invisible. Let the topaz be fixed in that position in which none of the two images should vanish, which is its depolarising position, and let the apparently single, though really double image, be viewed through a prism of Iceland spar. This prism will now form two images of the double images, each of which consists of two single images, and on turning the prism round one single image of each of the double ones will vanish in every quadrant; but two images will always remain, as if no change had been going on. The depolarisation of light is, therefore, the necessary consequence of the polarising faculty of the interposed topaz, and could not have existed unless the topaz had been a doubly refracting crystal. This theory is capable of the most satisfactory proof by substituting a rhomb of Iceland spar; a plate of agate, or a bundle of glass plates, in room of the topaz; or by transmitting a polarised ray along the axis which joins the obtuse solid angles of a prism of Iceland spar.

All bodies, therefore, that depolarise light must necessarily give double images polarised in an opposite manner, like those formed by calcareous spar. A great number of these bodies, such as rock crystal, topaz, &c. exhibit two images; but others, such as mica, amber, ice, gum arabic, caoutchouc, &c. present no direct indications of double refraction. In this latter class, therefore, the one image lies above the other, and they may, or may not, be produced by different refractive powers.

In regularly crystallized bodies, like topaz, &c. the neutral and depolarising axes of one plate uniformly coincide with the neutral and depolarising axes of all the rest, so that the crystal itself, though composed of numerous plates, has still two neutral and two depolarising axes. In substances, however, formed by successive layers, like caoutchouc and gum arabic, which have no neutral axes, but which depolarise light in every position, the first layer is deposited and crystallized so as to have two neutral and two depolarising axes; but there is no cause to determine that the second layer shall have its axes coincident with those of the first; so that after a number of layers are formed there will be a depolarising axis in every direction. This reasoning is capable of direct confirmation from an experiment, which I have described in my Treatise on New Philosophical Instruments. If we take several films of mica, and place them upon one another in any position but that which they had in the original crystal, they will be found to depolarise light in every position, like caoutchouc, from the want of coincidence among their neutral axes.

It follows, therefore, from this theory, that every layer of caoutchouc and gum arabic and horn is a doubly refracting and a polarising crystal; and that the agate when cut in one direction gives *two bright images*; whereas in another direction it is known to give only a bright and a nebulous image.

III. *On the coloured Rings produced by common and polarised Light.*

When a ray of common light falls upon a plate of topaz at an angle of 60° 38′ in a direction parallel to either of the longest diagonals of its primitive rectangular prism,* and is reflected from its posterior surface, it exhibits, when viewed through a bundle of glass plates, a series of *twelve* or *fourteen* brilliantly coloured eliptical rings, each of the first three or four containing all the prismatic colours, and the rest being composed only of pink and blue. The incident pencil is in this case polarised by reflection from the posterior surface of the topaz. The rings are therefore produced during the return of the ray from the posterior to the anterior surface. The only use of the bundle of glass plates is to extinguish the light reflected from the anterior surface, and prevent it from overpowering the coloured rings.

When the incident ray is polarised the rings are seen without the bundle of glass plates, and the transmitted light displays rings with colours complimentary to those which form the rings seen by reflection. These rings undergo many beautiful transformations by varying the circumstances under which they are produced, all of which I have endeavoured to represent in coloured drawings.

With a plate of topaz $\frac{103}{1000}$ of an inch thick, the conjugate diameter of the fourth red ring (the two central spots not being reckoned) subtends an angle of 18° 30′; and with a plate $\frac{227}{1000}$ of an inch thick, the angle subtended by the same ring is 8° 25′. Hence *the diameters of the rings are inversely as the thickness of the plates.*

I have discovered the same rings in the *agate*, in *rock crystal*, *mica*, *ice*, *amber*, *tartrate of potash and soda*, *sulphate of potash*, *prussiate of potash*, *nitrate of potash*, and in *acetate of lead*, melted and crystallized between two plates of glass.

By comparing the magnitude of the rings produced by topaz, ice, tartrate of potash and soda, and sulphate of potash, I concluded that their conjugate diameters were inversely as $(m - 1)^3$, m being the index of refraction. But the nitrate of potash forms a singular anomaly, as it produces in the direction of the axis of the hexaedral prism a series of miniature rings nearly *eight* times less than they should have been, according to the preceding law. They are, however, inversely as the thickness of the crystal These miniature rings form one of the finest phenomena in optics.

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* If this is not the primitive form of the topaz, which there is reason to believe, from some recent observations of Haüy, the two positions of the incident ray may be described in reference to the general form of the crystals of topaz. They are in a plane which cuts at equal angles the two faces that contain an angle of 124° 22′, and they are inclined 60° 38′ to the axis of the prism, or 29° 22′ to the surface of the laminæ.

IV. *Polarisation of Light by Reflection.*

1. *By Reflection from transparent Surfaces.*—The polarisation of a pencil of light by reflection from transparent bodies, discovered by Malus, is not, as he supposed, a general law, but seems to depend upon the relation between the quantities of reflected and transmitted light when the pencil is incident at the polarising angle. As this angle increases with the refractive power of the body, and as the quantity of reflected light also increases with the angle of incidence, the pencil reflected at the polarising angle from realgar is more than one half of the incident light. The consequence of this is, that the realgar is *incapable of polarising the whole of the pencil.* The same thing happens in the case of diamond and chromate of lead; and we are thus led to our views respecting the state of the rays before their approach to the refracting surface.

2. *By Reflection from Oxides of Steel.*—When a pencil of light incident at an angle of 63° upon a blue oxidated surface is viewed through a prism of calcareous spar, the images become alternately of a beautiful red colour, without any intermixture of blue, in every quadrant of the circular motion of the prism. The blue light, therefore, is polarised at that particular angle, and the red light which is transmitted through the film alone reaches the eye of the observer. At a greater angle of incidence, the red image becomes orange, and gradually approaches, as the angle increases, to the colour of the other image: at less angles of incidence than 63°, the red image becomes blue, and at last assimilates itself to the other image. Does it not follow from this experiment, taken in conjunction with other facts, that a part of the light which metals reflect has penetrated their surface, and has thus received the peculiar colour of the metal. The transparency of gold leaf, the green colour of the light which it transmits, and its power of polarising a portion of light by refraction, as I have found from direct experiment, confirms this conjecture, and would also lead us to conclude that the part of the light which has penetrated the metal must be polarised in an opposite plane to that which has not entered the body. It follows also from this experiment, that the refractive power of the blue oxide is about 2·00, a result almost the same as that which Dr. Thomas Young deduced from a very different method.

V. *Polarisation of Light by bundles of Glass Plates.*

As a brief abstract of these experiments was given in the last Number of the *Annals of Philosophy*, it is unnecessary to repeat them here. In my paper on this subject I have determined from numerous experiments the law by which the phenomena are regulated; and have pointed out the application of them in explaining the polarising power of doubly refracting crystals. A bundle of glass plates is, in fact, an artificial crystal that forms a single polarised image; and when the bundle is composed of a particular number of plates, it gives a bright and a nebulous image polarised in opposite planes, like the two images of the agate. By combining bundles of plates at different angles and in different planes, a number of curious results may be obtained, and a ray of light may be polarised in whatever direction it is incident upon the compound bundle. The most important result, however, is the power of a bundle of plates to polarise a beam of light *at any angle of incidence above the minimum polarising angle.*

VI. *Optical Properties of Alcohol of Sulphur.*

This singular fluid has the same relation to all other fluids as the diamond has to the precious stones. *It possesses a greater refractive power than any fluid that is at present known; and a greater dispersive power than any of them, except oil of cassia.* In its power of dispersion it ranks between balsam of Tolu and phosphorus. The value of $\frac{dR}{R-1}$, or the dispersive power, is 0·115. Its refractive power is about 1·680: but I do not give this result as correct, as I have not yet ascertained by what coloured ray the spectrum is bisected.

VII. *Optical Properties of Nitrate of Potash.*

This salt possesses the most remarkable optical properties of any substance that is at present known. Its various actions upon light are of the most anomalous and instructive nature.

1. It greatly exceeds calcareous spar in its double refraction, the difference between the two indexes of refraction in the spar being 0·146, and in nitrate of potash 0·180.

2. The least refracted image is nebulous; and, what will scarcely be credited, *it is formed by a refractive power the same as that of water.* The two images are polarised like those of all other doubly refracting crystals.

3. The dispersive power of the bright image is remarkably great, exceeding even that of flint glass; while the dispersive power of the nebulous image is extremely small. The difference indeed is so striking, that we have here an *ocular demonstration* of the existence of two dispersive powers in the same crystal, which I had formerly established by numerous experiments.

4. The coloured rings formed by this salt, and already noticed under a former head, form another optical anomaly equally surprising.

The carbonate of potash also gives a bright and an imperfect image, the least of which has a very low refractive power. Its indices of refraction are $m = 1{\cdot}482$, $m' = 1{\cdot}379$.

VIII. *Optical Properties of Carbonate of Barytes.*

This mineral resembles the agate in its optical properties. It forms two images; one of which is distinct, and the other indistinct, or nebulous. The following phenomena were exhibited by different prisms cut out of the same specimen.

1. One of the prisms formed four images, all of which were indistinct, and consisted of circular arches of nebulous light. The two middle ones were the largest and the brightest, and the other two were probably formed by reflection. All the images were polarised; but each of the two outer images was polarised in the same manner as the bright image farthest from it.

2. In another prism the most refracted image is of a brown hue, and so extremely faint, when compared with the least refracted image, that I at first imagined that I had discovered a polarising crystal that gave only one image.

3. In a third prism the most refracted image approximates to distinctness, but is never as luminous as the other.

The preceding phenomena exhibited by nitrate of potash and carbonate of barytes advance us an important step in the theory of double refraction. In the memoir which I have written on the subject I have attempted to explain the new views to which they lead.

I am, Dear Sir,
Your most obedient humble Servant,
Edinburgh, Jan. 12, 1814. DAVID BREWSTER.

ARTICLE VII.

On the Ventilation of Coal-Mines. By Mr. John Taylor, of Holwell-house, near Tavistock.

> "Qualeis expirat scaptesula subter odores?
> "Quidve mali fit ut exhalent aurata metalla?
> "Quas hominum reddunt facies, qualeisque colores?
> "Nonne vides, audisve perire in tempore parvo
> "Quam soleant."
>
> *Lucretius,* lib. vi. 810.

(To Dr. Thomson.)

SIR,

I READ the account of the dreadful accident at the Felling Colliery, in the first volume of your *Annals,* with peculiar interest, because the ventilation of mines at one time occupied a good deal of my attention.

The works in which I was engaged, and in which I succeeded in carrying perfect ventilation much further than had been attempted before, were copper-mines, in the western part of the county of Devon. In these, though the destructive effects produced by explosive gases are unknown, yet the labours of the workmen are frequently impeded by the deterioration of the air; this takes place when the levels extend beyond a certain distance, from a communication with the atmosphere.

It is usual in the mines of the district above-mentioned, and in those of Cornwall, to sink a greater number of shafts on this account than would otherwise be required; and as it was a very important object, in an undertaking of which I had the management, to save the expense of many deep shafts, which would have been necessary if ventilation had not been obtained in some other way, I endeavoured to accomplish this end, which, after some trials of unequal success, was at length effected.

For a description of the apparatus I employed, I beg leave to refer your readers to the Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce, for 1810. The paper in that volume is accompanied by a drawing of the machine, which has no peculiar merit besides that of combining simplicity of construction with considerable exhausting power at a small expense of moving force. The paper contains, besides, an account of its application to the tunnel of the Tavistock canal, and some calculations and suggestions as to its use in other cases.

I may state here, in addition, that as it had been effectually employed, for two years, when that paper was written, its use has been continued and extended in the four years which have since elapsed. That whereas in most similar works shafts are placed at every 60 or 70 fathoms on the course of an adit or tunnel, my shafts are distant from each other from 240 to 260 fathoms. That the spaces between each have been kept as free from any of the usual impediments as it is possible to conceive; so much so, that in the last shaft which was completed, it having been found difficult to sink it, on account of the quantity of water met with, more than half of it was done by working upwards from the tunnel; and that though this work was carried up perpendicularly between 30 and 40 fathoms, to meet the part which had been sunk; and though this was done at a point in the tunnel 230 fathoms distant from any opening to the surface, and where several men were working day and night, yet no inconvenience for want of good air was perceptible.

I should not have obtruded all this, however, on your readers, had not the account of another explosion in the Felling Colliery just met my eye, by which 22 men and boys are added to the victims of this destructive calamity, leaving 8 widows and 18 fatherless children; and this in the same spot where about eighteen months ago about 100 men were destroyed in a similar way. Nor should I probably have ventured to press a consideration of the subject, had I not been strengthened in an opinion which I ventured to give, in the paper to which I have referred; that my engine might be applied to collieries with effect, by the weight of your authority in some remarks of your own subjoined to the account of the former accident.

You have there shown that explosion cannot take place unless the carbureted hydrogen amounts to a certain proportion of the whole bulk of air, and that safety will be insured if the fire-damp could be withdrawn so as to prevent accumulation to more than a certain extent: and you suggest that some means might be devised for pumping it out as it is produced.

I beg leave to trouble you with an extract or two from the paper above alluded to, as containing my opinion at the time it was written, which opinion has been strongly confirmed by your observations on the subject.

" The size of the exhauster may always be proportioned to the demand for air; and by a due consideration of this circumstance, this engine may be effectually adapted, not only to mines and *collieries*, but also to manufactories, workhouses, hospitals, prisons, ships, &c.

" ——————— Not being practically acquainted with collieries, or mines, that suffer from peculiar gases that are produced in them, I cannot state from actual experiment what effect this machine might have in relieving them; but it must appear, I conceive, evident to every person at all acquainted with the first principles of pneumatics, that it must do all that can be wished, as it is obvious that such a machine must in a given time pump out the whole volume of air contained in a given space, and thus change an impure atmosphere for a better one. And in constructing the machine it is only necessary to *estimate the volume of gas produced in a certain time*, or the capacity of the whole space to be ventilated.———————

" ——————— With such a machine as this, if the dreadful effects of explosions of this air (fire-damp) are to be counteracted, it may be done by one of sufficient size to draw off this air as fast as it is generated, and by carrying the pipes into the elevated parts of the mine, where from its lightness it would collect."

These hints, I venture to think, fall in very nearly with what you have suggested; and my experience leaves no doubt on my mind that the object may be attained, by employing either my machine, or some better one producing a similar effect.

There is, I think, a strong reason why an apparatus of this sort may be more effectual in a colliery than any general system of ventilation by shafts, drifts, and currents of air.

A peculiar gas such as produces the destructive effects we hear of is, I should suppose, very probably generated at some one or more places in the mine. If this is the case, what do currents of air passing down one shaft and diverted by drifts and trap-doors till they find an exit at another, do more than this; that they mix with the gas, and carry it with them through all the circuit, dispersing it, though diluted more or less, over a considerable space.

On the other hand, a powerful exhausting engine may be brought to act at once on any particular point; and by a system of pipes, laid in a judicious manner, and provided with orifices which might instantly be opened at one place and closed at others, the gas might be pumped out from its source without mixing with the air of the mine, while the influx down the shafts would serve to keep the whole atmosphere of the mine in such a state as would ensure safety.

The extent of exhausting power to effect this would be a matter of no difficult calculation for any cure, and I have no doubt that other good means of obtaining it might be devised, as well as those I have practised. I shall only say in their recommendation, that they are simple, not expensive, not liable to frequent repair, and having but little friction may be worked by any first mover without laying such a burden on it as could be important in any undertaking of this sort.

I have, however, no particular reason for recommending this plan of my own: the object for which it was invented has been attained; its use is free for any persons that may wish to try it; and I need only say, that I shall cheerfully give every information about it, and the improvements it has since received, to those who may desire it; and so, I am convinced, would the agents of the Tavistock canal, where it is still in daily use.

One thing is clearly shown by the recent event at the Felling colliery, situate in one of the great coal districts of England, which is, that all the means they are at present acquainted with are inadequate to secure them from danger. This colliery is described as being excellently managed, and its apparatus and construction upon the best known principles, and yet with the dreadful catastrophe before them which happened only eighteen months ago, and with the most laudable exertion on the part of the proprietors to do all that could be thought of, another dreadful event has taken place, carrying misery to the widows and children of the workmen, and destroying the profit due to the enterprising spirit of the owners.

I understood that when my paper was read at the Society of Arts, it was said by a great coal owner that he despaired of any plan of ventilating collieries, however plausible, because none had hitherto succeeded. In the same manner, I was told by a friend, whose experience in mining is as great as that of most men, that I should find it impossible to ventilate the Tavistock tunnel with less than three times the number of shafts which I have since actually employed.

The failure of old plans seems to me rather a reason for experiment, than an argument against it; and I conceive it must now be admitted that there is no mode known in the collieries of producing complete ventilation; if there had, I take it for granted it would have been employed at the Felling works after the first accident, and a recurrence of the calamity would have been avoided.

I have practically employed in the mines in which I was engaged all the modes of ventilation that are commonly in use, and in many, or perhaps most cures of mines, there are sufficient; but they are often attended with more expense in the end than the one I have proposed, and I am satisfied will never be found so effective.

The principle I mean to insist on is the removal of the noxious air by some apparatus proper for the purpose, connected with pipes which are to be fixed so that they may be made to draw from any part or parts of the works, as occasion may require. How the exhaustion is produced is immaterial, so that the means employed be capable of a powerful effect. I thought of employing steam to produce a vacuum, and in a receiver which should thus draw air from the mine, and I had contrived an apparatus for doing the same thing by a stream of water alternately admitted and discharged from a proper vessel; but, though either of these schemes are practicable, there are good reasons for preferring the machine I adopted. As the capacity of exhaustion is the great thing to attend to, I shall just mention, that I conceive it easy to make an engine of this sort, which might be constructed by any mining carpenter at a moderate expense, and that might be worked at an easy rate, capable of pumping out 40,000 cubic feet of air in the hour.

If this should not be deemed sufficient, another equal in effect might be connected to the same pipes, to work in aid of the other, either constantly or occasionally.

A very important object might likewise be obtained by this machine, that is, that it could be made to furnish samples of the air of the mine at any instant on the surface, and delivered by a very simple apparatus into proper vessels in any convenient room or place where they might be immediately examined by proper tests, and thus the state of the air might be watched, and the increase or diminution of that which produces danger ascertained, and due notice could be given when there was cause for alarm.

A daily record of the changes that take place in this respect appears so desirable a thing connected with a plan of ventilation, that I should deem it important to render it as easy as this would do, and which the proprietors could command attention to by appointing one of their agents to make the proper experiments, and register them at stated periods.

Numerous schemes have been proposed for ventilating mines: many of these have been extremely frivolous, others were founded on an imperfect knowledge of the gases when chemical science was in its infancy, and some were defective for want of the necessary calculation as to the amount of effect requisite to be produced.

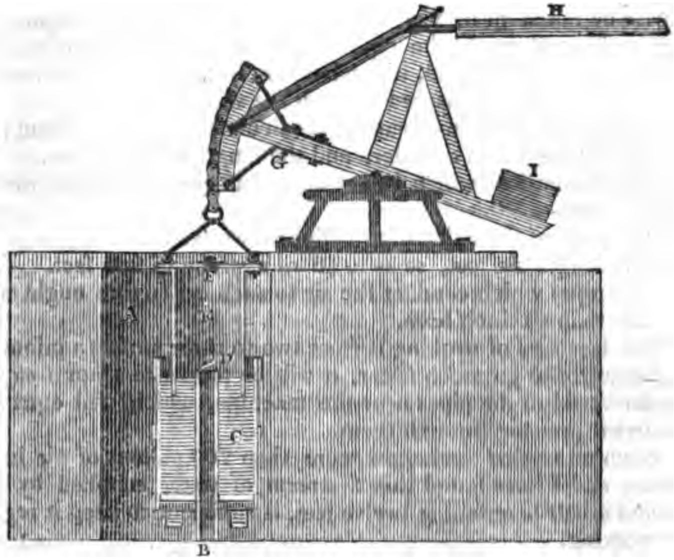
Where numbers have failed, it is not surprising that mere practical men should receive with doubt the proposals of new projects. I submit my own with deference to those who have the management of collieries, of which I profess to know very little, with this remark, that they may satisfy themselves of its use where it has long been employed, and that though I admit that the vitiation of the air takes place in these mines from a very different cause, and in a different manner, yet I conceive the cases are sufficiently analogous to justify a trial of it, where something is so much wanted.

I am, Sir,
Your obedient servant,
Stratford, Essex, Jan. 7, 1814. JOHN TAYLOR.

APPENDIX.

To give the reader an idea of the nature of this machine, we have thought it requisite to give a figure of it, and we extract the following description of it from Mr. Taylor's paper, printed in the Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce, for 1810:—

"I at last erected the machine, of which the annexed is a drawing; which, while it is so simple in construction, and requires so small an expence of power, is so complete in its operation, and its parts are so little liable to be injured by wear, that, as far as I can imagine, nothing more can be desired where such an one is applied. This engine bears considerable resemblance to Mr. Pepy's



gazometer, though this did not occur to me until after it was put to work. It will readily be understood by an inspection of the drawing, where the shaft of the mine is represented at A; and it may here be observed, that the machine may be as well placed at the bottom of the shaft as at the top, and that in either case it is proper to fix it upon a floor, which may prevent the return of the foul air into the mine, after being discharged from the exhauster: this floor may be furnished with a trap-door, to be opened occasionally for the passage of buckets through it.

" B, the air-pipe from the mine passing through the bottom of the fixed vessel or cylinder, C, which is formed of timber and bound with iron hoops; this is filled with water nearly to the top of the pipe, B, on which is fixed a valve opening upwards at D.

" E, the air or exhausting-cylinder made of cast-iron, open at the bottom and suspended over the air-pipe, immersed some way in the water. It is furnished with a wooden top, in which is an opening fitted with a valve likewise opening upwards at F.

" The exhausting-cylinder has its motion up and down given to it by the bob, G, connected to any engine by the horizontal rod, H, and the weight of the cylinder is balanced, if necessary, by the counterpoise, I.

" The action is obvious.—When the exhausting-cylinder is raised, a vacuum would be produced, or rather the water would likewise be raised in it, were it not for the stream of air from the mine rushing through the pipe and valve, D. As soon as the cylinder begins to descend, this valve closes, and prevents the return of the air which is discharged through the valve, F.

" The quantity of air exhausted is calculated, of course, from the area of the bore of the cylinder, and the length of the stroke.

" The dimensions which I have found sufficient for large works are as follow:—

" The bore of the exhausting cylinder two feet.

" The length six feet, so as to afford a stroke of four feet.

" The pipes which conduct the air to such an engine ought not to be less than six-inch bore.

" The best rate of working is from two to three strokes a minute; but if required to go much faster, it will be proper to adapt a capacious air-vessel to the pipes near the machine, which will equalise the current pressing through them.

" Such an engine discharges more than 200 gallons of air in a minute; and I have found that a stream of water supplied by an inch and a half bore falling twelve feet, is sufficient to keep it regularly working.

" A small engine to pump out two gallons at a stroke, which would be sufficient in many cases, could be worked by a power equal to raising a very few pounds weight, as the whole machine may be put into complete equilibrium before it begins to work, and there is hardly any other friction to overcome that of the air passing through the pipes."

ARTICLE VIII.

On the Electricity of Paper. By Mr. Walsh.

(To Dr. Thomson.)

SIR,

NOT knowing that the electrical properties of paper have hitherto been noticed in the high degree I have had occasion to remark them, I will trouble you with a short account of some experiments I have lately made on the subject: if they have any claim to novelty, it may prove interesting to the readers of your Journal to be acquainted with them, before the season favourable to the display of electrical appearances be at an end.

About four weeks ago, having rubbed with elastic gum a piece of foolscap paper, which I had previously held to the fire to dry some drawing made on it, I observed that it adhered strongly to the quire on which it was placed: as I attributed this to electricity, I resolved to repeat the experiment to more advantage; and for that purpose I warmed two sheets of the above-mentioned kind of paper, and having spread them out, laid them one on another on a well dried woollen table cloth, and then rubbed the uppermost of them for a few seconds: their adherence became considerable; and on forcibly parting them, a loud crackling noise was heard; in the dark this noise was accompanied by vivid flashes of light.

Forty sheets well dried before the fire, spread out, and placed over one another, were next electrified, by the rubbing of the uppermost, as before: their adherence to each other was then such that the two extreme sheets being held suspended by means of silk threads the intermediate ones did not slip from between them: the flashes and various ramifications of light emitted, in the dark, when these sheets were separated from each other, presented a beautiful appearance; and when they, after this separation, were held up suspended, currents of luminous fluid an inch long or more continued visible at each angle of the paper for a considerable time.

The above electrified pile served as a most powerful base for an electrophorus, and by its means sparks two inches in length were obtained from a metallic plate, made of two sheets of tin, each of which was not much above a foot square.

Two sheets of paper were coated, each on one side, with tin foil (gold paper will answer the purpose better), and a dozen uncoated sheets, well dried, placed between them; a few sparks from the electrophorus were discharged on the uppermost insulated sheet of the paper battery, which thus became sufficiently charged to give a smart shock.

If the sheets interposed between the coated ones had previously been electrified by rubbing, a long succession of slight shocks was obtained, with the only precaution, after each discharge, of separating the upper or insulated coated sheet from the under ones, and drawing a spark from it before it was put back in its place.

These experiments will succeed best when the bend or fold in the middle of each sheet will have been made to disappear (which may be done by wetting that part of the paper, and letting it dry under some weight, or by ironing), as then the sheets by lying more flat on one another will better exclude the air, which would in a great degree lessen their electrical energy.

The Indian rubber employed should be of the thick kind, and that side of it which bears the mark of the knife be brought in contact with the paper. This gum is in itself slightly electrical.

Drawings made on paper with lead pencil will, like those formed on glass with tin foil, become luminous when an electrical discharge passes through them: the whole surface of the black lead coating will sometimes appear a mass of light.

I am, Sir, your obedient humble Servant,

PHILIP WALSH.

ARTICLE IX.

On the Antilunar Tide. By John Campbell, of Carbrook, Esq. F.R.S.E.

(*Continued from* p. 131.)

THIS theory, it will be observed, rests on the supposition, that, at the *conjunction* the moon is acted on by the difference between the forces of the sun and earth; and that she is at the *opposition* affected by the *sum* of these forces. The other theory, similar to the theory of the tides, agrees in supposing the moon at the conjunction, to be affected only by the difference between the forces of the sun and the earth, but at the opposition, instead of ascribing the increasing curvature to the increase of gravity produced by the *joint action* of the two forces, it ascribes it to a diminution of gravity produced by their difference.* These theories cannot be both true, for they are contradictory to each other; but it will not require laborious investigation to determine which is entitled to preference.

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* M. Laplace states this very broadly. "Ainsi, de toutes les actions du soleil sur la lune, dans le cours de sa revolution synodique, il resulte un force moyenne, dirigée suivant le rayon vecteur lunaire, qui diminue la pesanteur de ce satellite." *Syst.* p. 228. How this acute philosopher should adopt an opinion, that in any situation, coincident attractions should act as opposite attractions, and in any degree destroy one another, would be inconceivable, were it not remembered, that his mind was directed to an analysis of intricacy sufficient to absorb all his attention. One would equally wonder, that the principles of pressure, or resisted attraction, did not occur to him as a certain and necessary cause of an antilunar tide, were he not persuaded of the truth of that philosopher's own remark on D'Alembert's discovery of the resolution of the laws of motion into that of equilibrium. "Que les ideés le plus simples sont presque toujours celles qui s'offrent les derniérès a l'esprit humain."

The general laws of gravitation need not be explained here. It seems to admit of no controversy, that coincident attractions, (attractions on the same side and in the same direction) however multiplied and varied as to motion or rest, do act with a force equal to the sum or aggregate of their attractions.* For, it is the general law of gravity, that every particle of matter gravitates towards every other particle of matter; that it is obstructed by no intervening body, or obstacle, and admits of no variation in the same matter, but from its different distances only, from that to which it gravitates. This general law, obviously determines the point where the bodies are at rest; for if two bodies, A and B, do act upon a third body, C, without any variation, in consequence of their relative attractions to each other, they must act on C with with the *sum* or *aggregate* of their attractions, that is, inversely, by their respective quantities of matter multiplied by the squares of their respective distances. But it also determines the point where the bodies are in motion: for, supposing them approaching or receding from each other, if any point in their path be taken, it is the same thing as to the bodies at that point, as if they were at rest, and the law will be there found to apply; but if this will hold, where *any point soever* is taken, it must hold in *every* point, and therefore the law, that bodies on the same side and in the same direction, act with the sum of their attractions, applies equally to bodies in motion as to bodies at rest. Let us apply these principles to the sun, the earth, and the moon. Let the earth, and the moon, at the period of opposition, be supposed mutually to attract each other, but the sun's attraction to affect the earth only, if the earth remain stationary, the mutual attraction of the earth and moon will not be affected by the superadded attraction of the sun on the earth. If the earth in these circumstances, be allowed to fall to the sun, then the attraction of the earth and moon will decrease as the squares of their distance shall increase; but if the moon be also projected in the same direction, and accelerated in the same proportion as the earth, their mutual attractions will remain unaltered. For, the gravitation of the different particles of matter to each other is independent of their relations to any other particles, and remains the same in all cases, except in as far as affected by difference of distance. "Gravity," as formerly stated, "is obstructed in its action by no intervening body, or obstacle, and admits of no kind of variation in the same matter, but from its different distances only from that to which it gravitates." To illustrate this, let us take the gravities of the sun, moon, and earth, at arbitrary values. Let the gravitation of the moon towards the earth, be indicated by the number 400; that of the earth towards the moon by 10, and let the force of the sun's action be indicated by 10,000, and be confined to the earth; then the moon would still be affected by the same force, 400; the earth by the sun's force, 10,000, and the moon's force, 10; and being supposed at the period of opposition, the effect on the earth would be 10,000 − 10 = 9990. Let the sun's attraction, still taken at 10,000, be now extended to the moon, all the bodies remaining fixed; then, while the earth continues to be affected with the same forces as before, that is, the *difference* of the attracting forces, the moon will be affected by the *sum* of these, or 10,000 + 400 = 10,400, and these forces will not be affected by any changes induced on these bodies, as long as they continue in the same direction, other than that produced by difference of distance. Let the earth and moon revolve round the sun, with a projectile force, respectively, such as shall exactly balance their gravitating forces: they will then describe circular orbits, and being each of them thereby kept at their original distance from the sun and each other, their mutual gravities will remain unaltered. But if this would really be the case in these circumstances, then there can be no truth in the position, "that, wherever the action of the sun would increase their distance, if they were allowed to fall towards the sun, there the sun's action, by endeavouring to separate them, diminishes their gravity to each other." And the theory founded on such a position must be erroneous also. It is obvious, that the additional movement of the moon in its orbit round the earth cannot affect the question; because if the sun's action would not diminish the gravity of the moon to the earth, were they moving in concentric circles round the sun (in which case the earth would, if allowed to fall freely, be separated from the moon, with an accelerating velocity,) there can be no law, which gives the *tendency* to such separation, the effects of *actual* separation. It must hold good in every case, or in *none*. It may be safely held, therefore, that the eccentricity of the moon's orbit is like the eccentricity of the orbits of the other satellites and planets, to be ascribed to the combination of forces and velocities, which increase and diminish in different ratios.

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* M'Laurin's Experiments, 4to. p. 276.

These objections to the principle on which the Newtonian theory is founded is equally strong, whether the application be made to the moon's orbit, or the swelling of the waters on the earth; perhaps therefore enough has been stated to warrant the rejection of the principle altogether; but as it is the foundation of a great fabric, it deserves more than ordinary consideration.

It may be remarked that considerable obscurity hangs over the principle itself. It is a plain and now an acknowledged law, that gravity decreases inversely as the square of the distance. It would be also easily understood, were we informed, that of two bodies falling from unequal distances towards the sun, the one nearest and most attracted would leave behind it the one least attracted; and that the action of gravity between these two bodies would be diminished by this separation in the ratio of the squares of their increasing distance; for all this follows, of necessity, from the general law. But this does not appear to be the effect inferred by M. M'Laurin, in the passage first above quoted. Indeed it is manifest that such a position could have been of no use to his theory of the moon's orbit and the antilunar tide. For these principles depend for their effects on absolute separation and increased distance. What then is the Newtonian principle? It can be nothing short of this, That an *endeavour* or *tendency* to separate two bodies, is equal to an *actual separation*, and equally diminishes their mutual gravities. Therefore, that an endeavour to separate the earth and moon, or the inferior surface of the earth, from the incumbent waters, will diminish the gravity of the moon and of the waters. The expression "endeavour to separate" is certainly a vague one. Supposing that the sun may be considered as endeavouring to draw the earth towards him, and that his endeavours with respect to the earth and moon in certain situations may not be equally vigorous, it is clear that the *separation* is not his object. It would only be a *consequence* of his action being unequally applied, and would at best be an insignificant consequence. But whatever the sun's endeavours might do, if they were successful, certainly they can effect nothing when they are unsuccessful. The endeavour to draw both the earth and the moon towards him is completely counteracted by the centrifugal force; and therefore as the *main* circumstance, of which the *separation* would be merely a consequent, does not happen, it is certain the consequent cannot happen. There being no separation, the endeavour to separate can produce no effect.

It will be said indeed that the earth and moon do actually fall to each other and to the sun, because they fall from the tangents of their curves. This however is a fallacy. If one of these bodies fell from the tangent, and the other did not, then, as there would be an actual separation, there would be a diminution of gravity, though not such a diminution as would support the disputed theory. But as the two sides of the earth and the moon do *all* fall from the tangents of their orbits; and do not, in fact, fall away from each other, their relative situations and gravities remain unaltered. But there is another fact in the relative situation of these bodies, equally conclusive against such effects being produced, by the circumstance of the earth falling from the tangent of its orbit. The eccentricity of the earth's orbit is so small, that it is always concave to the sun: when the moon is in opposition therefore, the earth is falling from her, and not towards her; it being impossible for a body to move in opposite directions at one and the same time, and the earth at that time, as at all times, falling from a tangent towards the sun. We come then, to this general result, that even were the earth and moon to fall freely to the sun, no other effect would be produced on their gravities, than that produced by the general law, which diminishes their gravities inversely as the square of their increasing distances; and that as these bodies do not fall freely to the sun, their respective gravities are not affected by any tendency to separation. Holding it then proved, that the eliptical figure of the moon's orbit is not produced by the endeavours of the sun, alternately to unite and divorce the moon from the earth; and in like manner, that the antilunar tide is not produced by the endeavour of the sun to draw the bed of the ocean away from the waters which repose on it, we shall proceed to the exposition of that cause which it is apprehended does produce the inferior or antilunar tide.

Mr. M'Laurin has somewhere considered the earth as composed of concentric coats or layers. Let us adopt this idea. By the moon's attraction, the particles of the hemisphere nearest to her, are drawn *from* the centre, while the particles or layers of the inferior hemisphere are drawn towards, or in the direction of, the centre. If the superior hemisphere could yield to this force, and fall towards the moon, the other no doubt would follow it. But, as the case stands, as the superior hemisphere does not fall away, the inferior hemisphere is resisted thereby, and the effects of the moon's attraction is to draw the concentric coats or layers together. Instead of separating them it unites them more closely.

Attraction resisted is equivalent to *pressure*. The phenomenon of water rising in a common pump, will afford a good illustration of this important fact. The effect of working the piston is the removal, from a portion of the surface of the water, of the superincumbent weight, or pressure of the atmosphere. The waters in the other parts, remaining subject to that pressure, and being prevented by the resistance of the earth at the bottom from moving in the direction of the attracting impulse, move towards that quarter from which the pressure is removed. They continue to accumulate on that spot till a column is formed which balances the pressure of the atmosphere. They rise into the pump. But the *pressure* of the atmosphere is nothing more than the *attraction* or *gravitation* of the atmosphere towards the centre of the earth, and the measure of the attraction is the measure of the pressure. We thus find a general law governing a vast variety of the operations of nature. We find that as in the case of *direct attraction*, (that is where the movement of the attracted body towards the body attracting is *not resisted*) the body *most* attracted leaves the body *least* attracted; so, *when attraction, or rather the motion of the body attracted, is resisted, the reverse takes place; the body least attracted leaves the body most attracted, but in an opposite direction*. Every ascent perpendicular from the centre ought, I am persuaded, to be ascribed to the operation of this law. To it appears to be owing the upright growth of vegetables, and to it I would ascribe the swelling of the antilunar tide.

The earth being supposed to have assumed that form which results from central attraction, and centrifugal force produced by rotation, the moon's attraction may be considered, as Newton has considered it, to be an additional force acting in parallel lines.

Suppose farther, that the earth were a hollow globe,* having a shallow covering of water on the interior and concave surface, as well as on the exterior and convex surface; and that the attractions on the interior and exterior waters on each side were equal. Then, as to the hemisphere nearest the moon, the waters on the interior and exterior sides, being affected by equal forces, would exhibit *opposite* effects. The exterior water being more attracted at the equator than at the quadratures, and being allowed to fall towards the attracting body, would be most drawn from the surface, and would raise a tide at the equator, while the interior waters, being *most attracted* at the *equator*, but being resisted by the bed of earth on which they rested, would be regulated by the laws of pressure, and would flow towards the quadratures, where the attraction or the pressure would be *least*. In like manner, as to the *inferior or antilunar* hemisphere, the quadratures being nearer the moon than the equator, and the water on the interior side being most attracted at the quadratures, and allowed to fall towards the moon, would in that place be *most drawn from the surface*, and rise into polar tides: whereas, the waters on the *exterior surface*, being resisted by that surface, would flow *from* the quadratures, where the pressure would be greatest, and accumulate at the equator, where the pressure would be least. The two exterior tides would rise near the equator, and the two interior tides would rise near the poles. Abstract now, the supposed circumstance of a hollow globe, and contemplate the earth as it is, a solid spheroid:—the same laws and the same phenomena remain. The lunar tide rises because it is *more* drawn *from the earth* at the *equator* than at the *quadratures*; and the antilunar tide rises, because it is *less* drawn *towards the earth* at the *equator* than at the quadratures. In the one case it is *direct attraction*, or attraction unresisted: in the other it is attraction resisted, or *pressure*. This theory may be further illustrated by the common example drawn from the principles of equilibrium. Suppose two columns of water, one at the equator, and the other at the quadrature, but connected so as the water might flow from the one to the other, they would, if equally attracted, stand at equal heights. If a weight be applied to the column of water at the quadrature, and not to that at the equator; or if the pressure of the atmosphere be withdrawn from the column at the equator, and not withdrawn at the quadrature, it is evident from these principles already noticed, which govern the rise of water in the common pump, that the water would accumulate in the column exposed to the *least pressure*, till the accumulated water in the one column balanced the water and additional weight, or pressure, in the other.

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* See plate XVI, figure 16, at page 48, where A represents the supposed shell or crust of the earth; B, the interior surface of the interior waters as unaffected by any attraction; C, the interior surface of the exterior water in the same state; D, the interior surface of the interior waters as affected by attraction; E, the exterior surface of the exterior waters as affected by attraction; F, F, F, parallel lines of attraction.

But the ocean may be considered as composed of an infinite number of such columns, and therefore the waters must accumulate by a progressive swell, from the quadratures to the equator, to balance the progressive increase of pressure from the equator to the quadratures.*

This theory has the advantage of simplicity, and simplicity, the offspring of unnerring wisdom and almighty power, is in general the companion of truth. It is also certain, that the cause to which the effect is attributed does exist, and that it must produce such an effect. If then the effect is equal to the phenomena of the antilunar tide, it must be the true and the only efficient cause; for another equal cause would produce a redundant effect.

On the principles above explained, the lunar and antilunar tides would coincide in all their appearances, and be of equal magnitude were their distances from the moon equal. Their difference, therefore, must be in proportion to the difference of the squares of their mean distances from the moon, and no more. The antilunar tide is produced by a force weaker than that which produces the lunar tide, as 1 is to 30, or thereabouts. If the medium lunar tide therefore be 10 feet, the antilunar tide on these principles will be 9 feet 8 inches.

ARTICLE X.

On the Limits of perpetual Snow in the North. By Leopold Von Buch, Fellow of the Royal Academy of Sciences in Berlin.†

FOR the first and only information which we possess respecting the limits of perpetual snow in the north, we are indebted to that eminent and able philosopher, Mr. Esmark of Kongsberg. It was made known in 1803, in the Danish newspapers, and in a later period in the Northern Archives for Natural History. (*Nordischen Archiv fur Naturkunde,*) by Professor Pfaff. Mr. Esmark informs us in that notice, that on the north and north-east declivities of Norway, he had observed the line of perpetual snow at the height of 3000 feet above the level of the sea, while on the south and west declivities of the same country, its height was no less than 7000 feet. This demonstrates that even in high latitudes this limit is still considerably elevated above the level of the sea. But the question in some measure still remains; What degree of latitude in Norway corresponds with this determination? For the land stretches from the 58th to the 71st degree of latitude. Neither is the estimation itself so precise as to convey an accurate idea of the snow-line. It is not possible for this line to vary so much upon the north and south sides of the same mountain. In narrow and precipitous tracts, the snow lies deeper than in other places. On such places the atmosphere is kept colder, and the approach of warmer air is resisted: this is the reason why snow lies in the *schneegruben*, (*snow-holes*) of the Riesengebirge, at a height of only 3700 feet above the level of the sea; though the line of perpetual congelation be 2000 feet higher than the most elevated summit of these mountains. For the same reason, masses of unthawed ice occur in the mountains of Jur, at the height of only 3,400 feet above the level of the sea.

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* This illustration is commonly applied to the Newtonian theory, and correctly too, because the Newtonian theory is equally founded on the difference in gravity between the equatorial and polar waters. The error lies in this, that it ascribes that difference to a greater diminution of gravity, whereas it truly arises from a smaller addition.

† Translated from Gilbert's Annalen der Physik, for 1812, vol. xli. p. 1.

The line of perpetual congelation is a crooked plane, which we conceive in the atmosphere, and the snow, situated at a greater elevation than this line, never melts. But it is by no means confined to the declivities of mountains. We seek for it on these declivities, because we do not know its real elevation in the atmosphere; and because it would be difficult for us to calculate it. The sun and the shade have a much smaller effect upon this line than upon the medium temperature of places. It is the business of naturalists to determine the form of mountains, the nature of the ground, and other causes which may make the snow in one place lie deeper than usual; while in other places, summits far above the usual limit of perpetual congelation are laid bare in summer; and by an examination of facts, to separate the general causes from those that are merely local. When this is done, we shall obtain a height for the line of perpetual congelation in the Norwegian latitude, not varying from itself several thousand feet, according as we take the north and the south sides of the mountains.

As Mr. Esmark has not fully solved the problem respecting the northern snow-line, I shall take the liberty to lay before the Academy,* what I have been able to collect respecting it. For an answer to this question, were it complete, promises a higher result than the mere solution of an important physical problem. If the curve or the snow-line over the earth's surface could be constructed from accurate rules, its height would probably indicate the temperature of all the places over which it passes, and thus make us acquainted with the law of the change of temperature, hitherto so little known.

It appears, at first sight indeed, that this law may be determined with great facility, by ascertaining the mean temperature of places by means of the thermometer. But that the application of the data furnished us by the thermometer, is attended with very great difficulty, is obvious from this striking but unfortunately true observation, that there are not above three, or at most four places on the earth's surface, with the mean temperature of which we are accurately acquainted.

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* This dissertation was read before the Berlin Academy, in 1809.

I.

No part of the great northern peninsula presents itself to observation above the snow-line. Among the many small mountains which run through Sweden, there are very few upon which snow lies in summer, and these must be sought for beyond the polar circle. So that eternal snow is as little known in Sweden, as in France or Germany. In Norway, on the other hand, snow mountains appear even in the lower latitudes, for Norway consists of a range of mountains, extending from one extremity of it to the other. This range in point of height is inferior to few, and in point of extent to none, of the European ranges of mountains. It not only extends without interruption for four degrees of latitune, from 58° to 62°; but during the whole of this length it extends to a breadth which far surpasses that of the Alps. And what shows that the Norwegian mountains are among the highest in Europe, is this; if we follow any of its valleys up to the top of the mountains, we come to a kind of plain, at the height of about 5000 feet above the level of the sea, extending in breadth eight, ten, or twelve German miles. The boors, who come up yearly with horses and cattle in great caravans from Hardanger on the west coast, over the mountains to Kongsberg, are obliged to spend the night in the desert on the top of the mountains. Hence it is obvious, that to travel over the mountains in one day exceeds their efforts. Where, either in the Alps or Pyrenees, are there mountain tops so broad, that we cannot traverse them in a few hours, and descend again into the valleys? The Norwegians very expressively call the great chain which divides their country, the *Langfielde*, or the *Storfieldt*, that is to say, the *long*, or the *great mountain chain*; for all other mountains, even those in Norway, disappear before it. As the different parts of the Alps are distinguished by the name of the countries and valleys over which they pass, in like manner the Norwegians give to the different parts of their *fielde* the names of the places situated on the declivity of the mountains. The *Byglefjeledt*, *Hardangerfjeldt*, *Fillefjeldt*, *Sognefjeldt*, are known to every Norwegian. As to the *Sevogebirge*, (the *Sevebierget*), we demand information concerning it in vain; for the Norwegian geographers justly consider it as more honourable to follow the natives of the country in naming these mountains, than to seek in Pliny for a name for a mountain hitherto unknown to the sailors of Christiana, Bergen, and Drontheim, and certainly equally unknown to the Romans!

Of the passes over these mountains none is better known than the pass of *Fillefieldt*, in the 61st degree of latitude; for scarcely any other is lower, narrower, or more easy to traverse; and scarcely any other is richer and more diversified in striking and sublime views. When in our road from *Christiana* we enter into the great valley of *Walders*, which is situated on the east side of Fillefieldt, we conceive ourselves to be in *Chamouni*, or in *Hasli*; and when from the heights of Fillefieldt we survey the western ocean, we renew the impression of the striking scenery in the straits of *Dazio* and *Giornico.* The number, the grandeur, and the height of the waterfalls; the dark road over the abyss; the bridges shaking like reeds; the rocks, the noise of the wild tumbling streams; altogether constitute so striking and wonderful a landscape, that Switzerland herself can exhibit few to be compared with it.

But as it happens in Switzerland, so also here, vegetation and living beings gradually disappear as we proceed up the pass. In the valley of *Halders,* to judge from the extensive woods of fine and tall spruce and Scotch firs, with which it is covered, there is nothing in the climate prejudicial to their growth. But in the pass at the top of Fillefieldt, we perceive only a few bushes of birch scattered here and there. On the sides of the pass even these disappear, and passing through moss and coarse grass, we reach the line of perpetual snow.

The spruce fir (*pinus abies*) goes to a considerable height above the valley of *Halders,* before it disappears. Both sides of the road between the churches of *Etnedal* and *Ourdahl* are covered with spruce fir bushes, though its height above the level of the sea amounts to 2876 feet. These, indeed, are no longer the spruce firs of the valley. Small, low bushes, have already acquired a great age. The little extent of their branches shows us, that at this height they have far exceeded the line which climate has assigned for their vigorous growth. At a small elevation above this height they disappear altogether.

That spruce and Scotch firs are not to be found in the country of *Vang,* and on the banks of the *Little Miosen,* a deep lake not far from the valley of *Walders* towards *Fillefieldt,* is a local phenomenon peculiar to that part of Norway, and belongs to the history of the distribution of plants in Norway. But neither the climate nor the soil have any thing to do with the phenomenon. *Vang,* and the small lake, lie only 1476 English feet above the level of the sea, and scarcely 400 feet above the lake of *Walders.* The fine corn-fields upon the declivity of the mountain of *Vang* demonstrate, that it is not the cold which has banished the spruce and Scotch firs from these mountains. As they grow at *Ourdahl* at a height of 2984 English feet above the level of the sea, so might they grow likewise not only at *Vang,* but at the summit of that alpine range, from *Miozen* to the greatest height of the pass: for *Nystuer,* a small inn, lies only 3,142 English feet above the level of the sea. The usual causes, which spread trees over the country, have failed in these high regions. The wind blows almost always from the west coast, and very seldom from the east, over the mountain range. Hence, perhaps, the reason why the whole west side of Norway is destitute of spruce firs. The wind may have even brought the seeds of spruces to the greatest height of the range, but the breadth may have been so great as to prevent these seeds from reaching any spot where they could vegetate. *Fillefieldt* itself, which from *Nystuen* to *Marystuen,* is only two German miles in breadth, must, when regarded in this point of view, be considerably broader. For the valleys towards the west sea are narrow winding glens, and the mountains which run along their sides continue quite to the sea.

Were the whole mountain range no higher than the pass between *Nystuen* and *Marystuen*, which scarcely amounts to 3000 feet above the level of the sea, we should be unwilling to class it along with high mountains. But the pass is a valley opening between high mountains, like the *Gotthardt*, or more accurately like the low *Brenner*, between eternal masses of ice. If we climb but a small way up the sides of this pass, we soon perceive mountains covered with snow, which appear at no great distance; and the summit of *Sule-Tind*, an insulated cone, rises like a colossal cupola, above all the neighbouring mountains. It is always considered as the highest place in the neighbourhood of *Fillefieldt*, and it commands all the declivities round about. Its sides are so rocky and rugged, that we ascend to the top of it with difficulty. The snow never leaves the foot of this mountain even in the middle of summer; and no doubt would cover its sides, if its steepness were not so great as to prevent it from lying.

On the 16th of August, 1806, at noon, the barometer stood at the same time,

On the top of *Sule-Tind* at............24·060 inches
Therm. 46·06°
At Christiana (20 feet above the sea) at..29·745 inches
Therm. 68°

Hence the height of Sule-Tind is 5876 English feet above the level of the sea; and 2664 English feet above the valley of *Fillefieldt*. The summit itself was quite free from snow, and consisted of loose blocks, on which not a single moss could be perceived. The circumference of this mountain is not considerable, and the same remark applies to its surface.

When we look to the south, we see no neighbouring mountain so high as this. But the mountains towards the north, separated from *Sule-Tind* by the valley of Fillefieldt, are higher. They may perhaps exceed it in height 200, or 300 feet, and reach in some places the height of 6181 English feet above the level of the sea. These mountains, with the separate names of which I am unacquainted, continue for a considerable space at a height more than 5000 feet above the level of the sea; and their summits are never entirely free from snow. For the wind cannot so easily drive the snow from these flatter declivities, and from the less insulated and broader summits into the valleys. Indeed the nearest valleys are only a few hundred feet lower than the mountains, and the snow scarcely melts in them. In such situations, the atmosphere is neither warmed by the melting of the snow, nor the action of the sun on the ground; and the temperature is always lower than upon the top of insulated cones, like *Sule-Tind*.

If the summit of *Sule-Tind*, which is but of small circumference, extended a German mile in breadth, in that case the snow would never leave it, and we should probably see a glacier formed upon its rugged declivity; for the summit of this mountain is elevated above the snow line.

If we observe the heights northwards of the pass where the snow begins to form a connected covering over the mountains, and compare them with the known height of *Sule-Tind*, we find them elevated about 5,200, or 5,300 feet above the level of the sea. Such in this place is the limit of eternal snow: not quite 900 toises (5755 English feet).

No glaciers are to be found in these mountains, nor in the neighbourhood of *Fillefieldt*. For in order to form glaciers, the mountains must run a much greater way into the snowy regions, than these mountains do. Glaciers require prodigious masses of ice, and a powerful pressure, in order to force the inferior parts of the ice into the lower valleys. Such masses of ice do not exist in those mountains which enclose the pass of *Fillefieldt* on the north side. Still further towards the north these mountains sink lower to a height not exceeding 4,500 feet: at that height there is a kind of plain formed, divided by flat valleys, which extends several German miles. It is called *Aardalsfieldt*, because it belongs to the parish of *Aardal* in *Sognefiord*.

This mountain plain, in the beginning of the last century, acquired a sudden and wide celebrity. Men had the boldness to commence a mine here above the clouds, and fine and rich pieces of copper ore found upon the mountain had even interested government in the success of the undertaking. It was necessary to sink the shaft through the snow; but not for any great depth. And instead of wood for the pits, which it would have been very difficult to have brought up the sides of so steep a mountain, they were under the necessity of employing ice. The water was allowed to rise in the pits and to freeze: the ice was then carried out, leaving behind pillars of it, by way of props, as there was no risk of its melting.[*] This mine had extended some miles, from the mountain to the foot of *Horunger*, a steep and high mountain above the pass of *Sognefieldt*, which separates the provinces of *Guldbrandsdal* and *Sogn* from each other. But the irregular distribution of the ore, and the difficulty of procuring it, soon put an end to the fond hopes of the projectors; and now nothing remains of the success of the undertaking, except the remembrance of the mountaineers; and some magnificent specimens of variegated copper ore, native copper, malachite, and native silver, in the Royal Cabinet of Minerals in Copenhagen, and in some other Cabinets in Germany.

II.

Farther south, and quite separate from the *Starfieldt*, there lies in the 50th degree of latitude a high mountain covered with eternal snow, deep in the interior of the province of *Hardanger*; but, like an island, surrounded by a bay of the sea. This mountain is seen many miles off by sailors, when on their way to Bergen they pass through the *Brommel-O* northwards, and over *Selboefiord*: and its aspect often strikes with astonishment sea captains from the southern regions of Europe, when they observe such a mass of snow towards the end of summer. The wonder is not diminished when we come near, and find glaciers and masses of ice upon an insulated mountain. The chain continues in a straight line from the north to the south more than twelve German miles. It rises rapidly at *Matre*, continues its whole length at the height of perpetual snow, and falls so rapidly at *Kinservig*, that masses of ice hang immediately above the bay called *Soefiord*. On the west it borders the great *Hardangerfiord*, towards the south the *fiorde* of *Aakne*, towards the north the *Samlenfiord*, and towards the east it approaches near the deep *Soefiord* lying between it and *Langfieldt*. There is only a narrow isthmus between *Aakne* and *Soefiord*, the greatest height of which, at *Vinterthan*, does not exceed 500 feet. This ridge, so completely surrounded by water, is called *Folge-Fonden-Fieldt*. *Ford* is the name given by the inhabitants to an ice plain; so that the name implies very expressively a *fieldt*, or mountain chain, covered with an uninterrupted layer of ice. Ship masters and Dutch charts have changed the name to *Fuglefang*, and in this corrupted form we find it in Ramus, and in other describers of Norway.

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[*] Deichmann Kongl. Vidensk. Selskaps Skrifter. xi. 148.

We are indebted to that well informed clergyman *Hertzberg*, in *Kinservig*, for an excellent account of this snow chain, which is situated within his view. He has ascended it often, and on different sides, and has carried a barometer to the summit. On the 25th of September, 1805, a portable barometer stood at

On the top of Folge-Fonden..	24·681 Eng. inches Therm. at 38·19°
At Reysaeter on the Soefiord..	30·109 Eng. inches Therm. at 53·4°

This gives the height of *Folge-Fonden*, according to Laplace's rule, 5421·9 English feet above the level of the sea. The barometer stood on the snow, but not at the greatest height of the mountain. Mr. Hertzberg thinks that the additional height may amount to 213 feet; so that the whole height of the mountain is 5634·9 English feet above the level of the sea. In the whole extent of this mountain chain there occur no separate eminences; the whole is one immense snow cupola, without division or valley, similar to the Buet upon a small scale. Even on the summit no ice is to be found. If we penetrate through a hard crust of snow about a foot thick, we come to white snow of an unknown depth. Masses of ice appear first on the declivity of the mountain: among these none are greater than those which precipitate themselves down the west side. In that quarter in the parish of *Quindherred*, a smaller bay, the *Molang fiord*, makes it way into the mountain. At the end of the *fiord* the valley of *Bondhuusdal* opens; and in the back ground of this valley there is a glacier, similar to one of the glaciers of Grindenwalds. The ice pushes evidently from under the snow, which covers the whole mountain, and passes without interruption to the deep. According to Mr. Hertzberg, its under side is about 1130 English feet above the sea. The considerable brook at *Matre*, on the southern extremity of the mountain, owes its origin to a similar glacier.

Folgefonden presents us with all the appearances of a mountain chain, which not only reaches the snow line, but extends a considerable way above it; yet the greatest height of this mountain is inferior to that of *Fillefieldt*, which only reaches the snow line. Mr. Hertzberg is of opinion that the snow line on *Folge-Fonden* cannot be placed higher than 5115 English feet above the level of the sea: and other circumstances confirm this opinion. The *Melderskin*, a high peak above *Rosendal* in *Quindherred*, never is destitute of snow upon its summit. Upon its east side there is even a commencement of a glacier; yet its height, ascertained by the barometer, is only 4860 English feet. The same appearances are observable at *Solen-Nuden*, one of the summits with which *Folge-Fonden* falls into the Soefiord above *Ulensvang*; yet *Solen-Nuden* is only 4796 English feet high. Flakes of snow lie even upon *Age-Nuden*, though that mountain is only 4587 English feet high.

One might easily account for this sudden sinking of the snow line, by the neighbourhood of the sea. The almost constant fog over the outermost islands, the covering of clouds, the rain, exclude the influence of the sun from the ground. The amount of the heat of the summer months, during which alone the snow melts, is in these wet places much smaller than deeper within the land and on the mountains, where the cooling of the sea air being less rapid, the production of vapour is less striking; but this explanation could not be applied with accuracy to *Folge-Fonden*. This mountain is probably too far removed from the action of the main ocean; for according to the five years' observation of Mr. Hertzberg, at *Malmanger*, in *Quindherred*, the months of June and July fall short of the heat of the same months at Upsala; but the case is different in harvest, and in the early part of the year. The temperature of September at Upsala is 52¼°; at Malmanger it is 56¾°. The heat of October at Upsala is 43·63°; at Malmanger, 48·9°. The same thing holds in the beginning of the year. The temperature of April in Upsala is only 40·01°; while at Malmanger it is 44·4°. In May the temperature of Upsala is 49·01°; at Malmanger, 52·81°. The effect of summer is also greater at *Hardanger* than on the east side of the great mountains, and farther from the sea; and should this difference in the temperature of spring and harvest diminish as we approach *Langfielden* on both sides of the mountains, as the warm south and west wind may be conceived to produce a greater effect there than upon the more remote country of Upsala; yet this would not explain the great difference in the height of the snow line above *Hardanger* and on *Fillefieldt*.

With greater probability, may we ascribe the lower level of the snow line in this place to the great accumulation of snow. An uninterrupted field of snow extending twelve German miles in length, and at least two in breadth, must obviously cool the atmosphere. The cold air sinks down along the steep declivities, and draws the snow line along with it. Saussure observed the same thing in the Alps,* and Ramond in the Pyrenees. This observation has led the last named philosopher to the fine observation that the snow line along the breadth of the Pyrenees constitutes a curve with its convexity turned towards the earth, and whose apex is at the middle distance between the two sides of the mountains. The warm air of the low country acts towards the sides of the mountains, and makes the snow line stand high. Towards the middle the great ice masses with which the mountains are covered cool the air, and the snow line sinks lower.

The snow line appears likewise to rise higher in Hardanger the farther distant you go from the *snow field* of *Folge-Fonden.* On the 15th of September, 1806, Mr. Herzberg and I ascended *Revilds-Eggen* immediately above *Ulensvang,* and only separated from *Folge-Fonden* by the Soefiord. This mountain, a continued ridge, and the first stage from *Long fieldt,* rises immediately above *Folge-Fonden,* so rapidly that the whole mass seems to constitute a perpendicular wall over the *fiord.* The bay constitutes a narrow valley, from which vertical cliffs above 4000 feet in height immediately rise. This appearance is so formidable, that the imagination will hardly permit us to approach the edges. If *Lauterbrunnen* in Switzerland were a *fiord,* and the steep rocks at its side the shore, the impression made upon us by *Soefiord* would by no means be equalled. But we might perhaps find some similar places in Norway itself, or on the north-west coast of America. The barometer stood on

Revilds-Eggen at.................	25·311 Eng. inches Therm. 45·5°
At *Ullensvang* on *Soefiord* at	29·956 Eng. inches Therm. 52·25°

This gives us the height of the mountain 4574·2 English feet. Now no patches of snow lie on the top of this mountain. The chain rises still some hundred feet higher, and continues of that height for eight or ten German miles, yet large fields of snow are nowhere to be seen; but only patches in the flat valleys, which divide the chain. In the middle of this extensive, bare, desert, and cold mountain plain, the *Hartoug* or *Hoarteig* stands like a tower. This is a rock under which the road from *Hardanger* to *Kongsberg* passes. It rises fully 800 feet above the plain, and is elevated at least 5542 English feet above the level of the sea; yet its base is free from a covering of snow, and only some patches appear on its summit. Here then, in the same latitude, and not far from *Folge-Fonden*, the influence of *Fillefieldt* on the height of the snow line is fully confirmed; for in this place there is not a large extent of surface covered with cold snow and masses of ice.

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* Saussure's Voyages, § 942. He conceives that the cold produced by the snow fields, and the water from melted snow, are capable of sinking the snow line more than 600 feet below its level, in mountains of less height, and less covered with snow.

Hence we cannot err far if we lay down 5542 English feet, or 1847 yards, as the height of the lower snow line in the latitude of 61°.

III.

There is another mountain range in the same latitude almost as extensive and fully higher than *Folge-Fonden-Fieldt*, which goes in a different direction from the great chain, and is scarcely connected with it. This is *Justedals Eisberge*, on the north side of *Sognefiord*, and almost above the mountain of *Fillefieldt*. We do not know the height of this mountain, and it was not in my power to ascertain it. On the road from *Justal* to *Lyster*, on the *Sognefiord*, I saw the Scotch fir beginning to disappear upon the mountain of *Vigedal* at the height of 2425·6 English feet. From a good many observations, I consider the snow line to be elevated above the line of Scotch firs 2771 English feet. This observation would induce us to fix the snow line in this place at a greater elevation than 5000 feet; so that it would not differ much from its elevation at *Fillefieldt*. In the interior of this *snow-field* it is probable that the snow line sinks lower, just as it does at *Folge-Fonden*; for the snow tract of *Justedal* extends about ten German miles in length, and rather more than two miles in breadth. If any conclusion can be drawn from a bare inspection, the height of this chain cannot be less than 6400 English feet.

Nowhere in Norway are there greater or finer glaciers than those which proceed from this chain. They are well known to the inhabitants by the name *Jis-Braeer*, and at times dreaded by them; for in their motion they become more rapid than the glaciers of Switzerland. In the year 1744 the few persons who inhabited these valleys complained that they were no longer able to pay their taxes, because the *Braeer* had rushed upon their fields, and covered them. This statement was not credited: surveyors and excisemen (*sorenscriver* and *foged*) were sent as commissioners to measure the distance of the middle of *Milvirsdal* from the foot of the nearest glacier; and it was ordered that this measurement should be repeated every three years, to ascertain whether or not these glaciers were advancing: three years after, the same commissioners went to repeat their measurement, and were not a little astonished to find neither fields nor houses. The *Jis-Braee* had advanced prodigiously, the inhabitants were gone, and their possessions were buried under the ice.* Equally destructive, about the same period, were the glaciers in *Kronda*, a valley which, as well as *Milvirsdal*, lies at the extremity of the great *Justedal*. But at what other place do we see such glaciers? In *Krondal* they appear as a huge, dazzling, white carpet, fastened on both sides to mighty rocks. When the glacier has reached the valley, it still continues to push on, like the glaciers of the Rhone, and shoves a high *Moraine* before it; and from the sides of the valley new glaciers come down, some to the bottom, some only one-half or one-third of the way. There is at present an inhabited house, *Bersetgaard,* quite in the neighbourhood of this glacier; and unless the great *Moraine* prevented it (for it is the size of a mountain), the ice would, without doubt, reach the corn-fields. The foot of this remarkable glacier is 1592 English feet high; and the church of *Justedal,* in the middle of the valley, is only 680 feet above the level of the sea.*

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* Thaarup's Magazin fur Statistik, 1802, ii. B. 1 H.

(*To be continued.*)

Article XI.

Astronomical and Magnetical Observations at Hackney Wick.

By Col. Beaufoy.

Latitude 51° 32′ 40·3″ North. Longitude West in Time $6''\frac{82}{100}$.

Feb. 1, Immersion of ν Gemini	11h	12′	14·5″	App. Time
	11	26,	14·7	Mean. Time
Emersion	12	13	18·4	App. Time } H.W.
	12	27	15·2	Mean Time } H.W.
Feb. 4, Immersion of Jupiter's 1st Satellite	9	10	26·9	Mean Time at H.W.
	9	10	33·8	Ditto at Greenwich
Feb. 16, Immersion of Jupiter's 2d Satellite	11	34	33·8	Mean Time at H.W.
	11	34	40·4	Ditto at Greenwich

Observation of the 16th uncertain to five seconds.

Magnetical Observations.

1814.

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Jan. 19	8h	55′	24°	15′	24″	—h	—′	—°	—	—″		Discontinued from the shortness of the days.
Ditto 20	—	—	—	—	—	1	55	24	20	41		
Ditto 21	8	55	24	14	28	1	55	24	19	28		
Ditto 22	8	55	24	15	39	1	55	24	19	20		
Ditto 23	8	55	24	14	37	1	42	24	19	39		
Ditto 24	—		—	—	—	1	55	24	19	00		
Ditto 25	9	00	24	19	38	1	45	24	23	12		
Ditto 27	—	—	—	—	—	1	55	24	22	04		
Ditto 28	—	—	—	—	—	1	55	24	21	27		
Ditto 30	8	45	24	16	39	1	55	24	20	57		
Ditto 31	8	45	24	14	53	1	50	24	17	35		

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* These remarkable glaciers, on account of their distance probably, are so little known in Denmark, and even in Norway, that a celebrated naturalist and traveller through a great part of Norway (Professor Hornemann) considered me as guilty of an error when I spoke of glaciers in Norway, proceeding, in his opinion, from my ignorance of the language (*Scandin. Litter. Selscabs scrifts,* &c.) Yet these remarkable spots may be visited from Christiania, or Bergen, without much difficulty; and they are as well entitled to a journey from Copenhagen as the valley of Chamouny.

Mean of Observations in Jan.	Morning	at	8h	52′	Variation	24°	15′	05″	West.
	Noon	at	1	58	Ditto	24	19	03	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Dec.	Morning	at	8	58	Ditto	24	17	21	West.
	Noon	at	1	53	Ditto	24	19	49	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	42	Ditto	24	17	42	West.
	Noon	at	1	54	Ditto	24	20	24	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	Ditto	24	15	41	West.
	Noon	at	1	59	Ditto	24	22	53	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	Ditto	24	15	46	West.
	Noon	at	2	02	Ditto	24	22	32	
	Evening	at	6	03	Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	Ditto	24	15	58	West.
	Noon	at	2	02	Ditto	24	23	32	
	Evening	at	7	05	Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	Ditto	24	14	32	West.
	Noon	at	1	50	Ditto	24	23	04	
	Evening	at	7	08	Ditto	24	13	56	
Ditto in June.	Morning	at	8	30	Ditto	24	12	35	West.
	Noon	at	1	33	Ditto	24	22	17	
	Evening	at	7	04	Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	Ditto	24	12	02	West.
	Noon	at	1	37	Ditto	24	20	54	
	Evening	at	6	14	Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	Ditto	24	09	18	West.
	Noon	at	0	59	Ditto	24	21	12	
	Evening	at	5	46	Ditto	24	15	25	

Magnetical Observations continued.

Month.		Morning Observ.					Noon Observ.					Evening Observ.	
		Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Feb.	1	8h	45′	24°	14′	16″	1h	55′	24°	16′	37″		Discontinued, from the shortness of the days.
Ditto	2	8	55	24	16	42	1	45	24	21	58		
Ditto	3	8	45	24	12	32	—	—	—	—	—		
Ditto	4	8	40	24	12	26	1	30	24	20	20		
Ditto	5	8	50	24	12	34	—	—	—	—	—		
Ditto	6	8	45	24	12	56	1	47	24	20	48		
Ditto	7	8	50	24	12	11	1	50	24	14	58		
Ditto	9	8	40	24	12	30	1	55	24	20	50		
Ditto	10	8	50	24	12	19	2	30	24	24	06		
Ditto	11	8	40	24	17	55	1	50	24	22	55		
Ditto	12	8	55	24	14	27	1	55	24	17	46		
Ditto	13	8	55	24	14	31	1	47	24	18	05		
Ditto	14	8	55	24	15	13	1	55	24	23	42		
Ditto	15	8	50	24	14	41	1	50	24	20	01		
Ditto	16	8	52	24	18	07	2	15	24	19	10		
Ditto	17	8	40	24	14	16	1	25	24	21	33		

Feb. 8.—The wind blew very hard from the W.N.W. The needles vibrated at intervals 10 minutes, for which reason I have not set down the observation.

Rain fallen	Between noon of the 1st Jan. Between noon of the 1st Feb.	1·320 inches.

ARTICLE XII.

ANALYSES OF BOOKS.

Memoires de l'Academie Imperiale des Sciences de St. Petersbourg. Tomes i. ii. iii. St. Petersbourg. 1809, 1810, 1811.

The Imperial Academy of Petersburgh have always made mathematics the most prominent department of their memoirs. On that account, the present volumes, though of a very considerable size, and containing abundance of valuable papers, are not very susceptible of abridgement. I shall satisfy myself with giving the titles of the mathematical papers. This will apprize the mathematical reader of the kind of articles they contain, and the probability of his finding any particular topic in them, of which he may be in search. Of the other papers, as far as they come within the plan of this work, I shall endeavour to give an abridged but tolerably accurate view.

VOL. I.

1. On the Resolution of compound Fractions into simple ones. By L. Euler. P. 3.

2. Elucidations of the geometrical Problem respecting the Quadrisection of a Triangle, formerly treated of by James Bernoulli. By L. Euler. P. 26.

3. A complete Solution or the Problem of the Quadrisection of a Triangle, by two straight lines cutting each other. By L. Euler. P. 49.

4. A Decade of geometrical Problems in the Inverse Method of Tangents, relating to the Radius of Osculation. By Nicolaus Fuss. P. 88.

5. Integration of some Formulas of differential Angles. By N. Fuss. P. 138.

6. A Method of converting any given Series into continued Fractions. By C. F. Causler. P. 156.

7. Attempt to demonstrate the Principles of Virtual Velocity. By B. Viscovatoff. P. 178.

8. Of the important Use of continued Fractions in the Integral Calculus. By C. F. Causler. P. 181.

9. A direct and inverse Demonstration of the general Principle of Equilibrium, in an elementary Manner, with the Application of this Principle to Machines. By S. Gourieff. P. 195.

10. Of the general Method of reducing all Kinds of Quantities to continued Fractions. By B. Viscovatoff. P. 226.

11. Hypothetical Law of the Inclination of the Needle in all Parts of the Earth. By W. L. Krafft. P. 248. This is a simplification of the well known Theory of Biot, published in 1804.

12. Solution of the following Diophantine Proplem: To divide a given number into any number of parts, so that their sum (one

of the parts being taken away) shall constitute a square. By C. F. Causler. P. 271.

13. Astronomical Determination of the position of some Towns of the Russian Empire. By F. S. Schubert. P. 283. The following are the latitudes and longitudes determined in this paper.

	N. Latitude.			Longitude in Time.			
Nizhni-Novjorod	56°	19′	43″	2^h	48′	33·4″	E. from Paris
Casan	55	47	51·4	3	8	3·6	
Perm	58	1	13	3	36	25	
Catherineburg	56	50	38·2	3	53	20	
Tobolsk	58	11	42·6	4	23	2·9	
Tara	56	54	31	4	47	0·2	
Tomsk	56	29	38·9	5	31	18·4	
Krasnoyarsk	56	1	2	6	2	30·1	
Nizhni-Oudmsk	54	55	22·4	6	26	46·1	
Irkoutsk	52	16	41	6	47	25·2	

14. Observation of six Kittens born adhering together. By N. Ozeretskovsky. P. 313. Six kittens were adhering together by the umbilical cords. One was dead, the others had lived four days, and were in good health.

15. A Chemico-botanical Description of the Equisetum Arvense. By T. Svelovsky. P. 316. The author gives a botanical description of this plant, and then a kind of chemical analysis of its tubers, which he says are of the size of a nutmeg. 120 grains yielded 14 grains of gluten, 22 grains of starch, and a black saccharine matter.

16. Method of catching the Tetrao Tetrix (black Grous) in Russia. By N. Ozerelskovsky. P. 321. Stakes pointed at both ends are driven into the ground, approaching near each other at the bottom, but diverging at the top, so as to resemble a funnel, or inverted cone. To the top of each stake is tied an oat straw with the grain on it. A long stake stands up in the middle of this machine, likewise crowned with oats. To this is attached a horizontal stick, vacillating freely within the cone. The birds come to eat the oats and light on this stick. It gives way and lets them fall into the cone, where, not being able to use their wings, they remain prisoners, till the proprietor of this singular trap comes and takes them out.

17. Description of the species of *gallium* found at the Cape of Good Hope. By C. P. Thunberg. P. 326. Eight species of this genus of plants are described, several of which are figured.

18. A systematic Exposition of the Minerals of Finland. By B. Sewerguine. P. 332. About 70 species are mentioned and described according to the Wernerian method. The author's journey was hasty, and nothing of much importance can be gathered from the paper. His description is imperfect, and he does not appear to have been acquainted with the principles of geognosy.

19. A botanical Description of a new Species of Myosotis. By

J. H. Rudolph. P. 349. He gives the species the name of *myosotis cileata*. It grows among the Altaic mountains.

20. Anatomical Observations on an uncommon Variety of certain Muscles of the Human Body. By P. Zagorsky. P. 355. The muscles, that varied from their usual position, were the *latissimus colli*, the *complexus*, the *interspinales cervicis*, the *crico-thyreoideus*, the *trapezius*, and the *biceps brachii*.

21. On the Mines in the Neighbourhood of the River Toura in the Uralian Mountains. By B. Sewerguine. P. 360. These mines are all of copper, and several of them consist chiefly of native copper. The paper gives us no information respecting the kind of rock in which they occur. But it is probably primitive, as sienite is mentioned as existing in one of them.

22. Botanical Description of a new Species of *Fumaria*. By J. H. Rudolph. P. 379. This new species, named *fumaria peregrina*, grows in Siberia.

23. Four Anatomical Observations of singular Aberrations in the Arteries. By P. Zagorsky. P. 384. These observations relate to the arch of the aorta, the external carotid, the axillary, and femoral arteries.

24. On a new gigantic Species of Actinea, observed in the Harbour of St. Peter and St. Paul at Kamtschatka. By G. T. Tilesius. P. 388. The paper contains a description and figure of this species, called *Actinea priapus*, on account of the striking resemblance which it bears to the penis of a horse. Tilesius gives likewise an anatomical dissection of the species, and a scientific account of the genus of actinea.

25. A Botanical Commentary on the genus Ziziphora. By J. H. Rudolphus. P. 423.

26. An Examination into the *fatuous* Acid of Winterl. By A. N. Scherer. P. 438. 'Winterl supposed that all acids owe their acidity to a common principle. When they contained this principle he called them *living* acids; when deprived of it, *dead*, or *fatuous* acids. When an acid combines with a base it parts with its principle of acidity; when it separates, it again unites with this principle. When one acid expels another from a base, a double decomposition takes place; the principle of acidity separates from the one acid, and unites with the other, while the base does the same. Winterl asserted that if carbonic acid were expelled from a base by heat, it would want the principle of acidity, and would not therefore exhibit the usual properties of an acid. The object of this paper is to prove by experiment, that this assertion is ill founded.

27. A Description of some new Species of Animals in the Museum of the Academy. By A. Sevastianoff. P. 443. Four animals are described; two quadrupeds from Botany Bay sent to Petersburgh from London, the lacerta interrupto-lineata, and the chætodon quadrifaciatus.

28. Meteorological Observations made every Hour between the

Tropics in the South Sea, to examine the Oscillations of the Barometer. By Messrs. Langsdorf and Horner. P. 450. From these observations made with great care, it appears that regular oscillations take place in the barometer between the tropics. It is lowest at half past three in the morning, highest at half past nine; becomes again lowest at four in the afternoon, and rises again till ten, when it is at its maximum. The difference of elevation amounts to about ·06 or ·07 inch.

29. On the Principles of the Science of Government. By H. Storch. P. 489.

30. Developement of the Principle of natural Liberty; or a summary Exposition of the Doctrine of Adam Smith, respecting the Object of Government. By H. Storch. P. 516.

31. Statistical Description of the Salt Lakes of Russia, with a preliminary Discourse on the Commerce of Salt in that Empire. By C. T. Herrmann. P. 593. This is a curious paper. The Russians obtain their salt chiefly from salt lakes in the Steppes and the Crimea, in which the salt crystallizes during the summer months. Since the beginning of the last century, Government have had a monopoly of the salt, and have furnished it to the people at a moderate price, from the benevolent design of letting the subject obtain this necessary article at the lowest possible price. But the business in the hands of government is attended with considerable loss, which is every year increasing. The lake Elton in north latitude 51½° near the Wolga, furnishes by far the greatest quantity of salt. But these lakes are mostly situated in deserts, and the expense of the carriage of the salt is enormous and constantly increasing.

32. On the actual State of Agriculture in Russia. By C. T. Herrmann. P. 662. Russia is an agricultural country. The northern and southern parts are not so well adapted for the purposes of the farmer, as the middle regions; the first are too cold, the second consist of the Steppes, which are uninhabitable on account of the want of wood and water. The grain chiefly cultivated consists of rye, wheat, barley, and oats; besides flax and hemp, which are very much attended to. The average return is five times the quantity of seed sown. The following little table will give some idea of the relative proportion of land cultivated, covered with wood, meadow, waste land, &c. in Russia.

Suppose the extent of European Russia to be 1, then there are

Cultivated land	0·15022,	or $\frac{3}{20}$
Woods and forests	0·42973,	or $\frac{3}{7}$
Meadows	0·03043,	or $\frac{1}{33}$
Courts and gardens	0·00525,	or $\frac{1}{190}$
Roads, canals, and rivers	0·05000,	or $\frac{1}{20}$
Waste land	0·33436,	or $\frac{1}{3}$

(An Account of the second Volume in our next.)

ARTICLE XIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

ON Thursday, the 27th of January, a paper by Dr. Brewster was read, on the *Polarisation of Light by Refraction.* Dr. Brewster has ascertained that when light is transmitted obliquely through any transparent body whatever, part of it suffers polarisation. The quantity of polarised light varies as the cotangent of the angle of incidence; and there is always a particular angle depending upon the refractive power of the body at which the emergent light is wholly polarised. When the light is transmitted through a number of parallel plates, the cotangents of the angles of polarisation are always to one another as the number of plates employed; and the number of plates multiplied by the tangent of the angle at which they polarise light is a constant quantity.

On Thursday, the 3d of February, a paper by Sir Humphry Davy was read, entitled *Some further Experiments on Fluorine, and on some other Objects of chemical Investigation.* This paper consisted, in fact, of three distinct subjects, treated of in succession. 1. Several further experiments were related, made in order to obtain fluorine in a separate state, but they were all unsuccessful. Fluorine has such a tendency to enter into combinations, that no vessels can be procured upon which it does not act. Most of our readers are probably acquainted with Sir H. Davy's opinion respecting fluoric acid. He considers it as a compound of hydrogen and an unknown supporter of combustion, which he calls *fluorine.* This fluorine has the property of combining with the base of silica and with boron, and it forms an acid with each. The fluates are compounds of fluorine and the metals, which constitute the bases of the salifiable bodies. A number of experiments to determine the proportions of the constituents of these bodies were detailed. Fluor spar, when heated with sulphuric acid, increases in weight from 100 to 175·4; but it was requisite to repeat the process eight times to obtain the full effect. If we suppose fluor spar to be a compound of fluoric acid and lime, this result gives us its composition as follows:—

Lime	73·661
Fluoric acid	26·339
	100·000

But if its constituents are fluorine and calcium, then its composition is—

Calcium	53·313
Fluorine	46·687
	100·000

According to this statement, an atom of fluorine will weigh 2·294 (supposing an atom of calcium to weigh 2·62*). The analysis of several other fluates were given, but we cannot venture to state the numerical results with accuracy from memory. The reader will be under no difficulty in supplying this deficiency, if he considers them as compounds of an atom of fluorine and an atom of the metallic bases.

2. The second part of the paper was on *silica.* A number of unsuccessful attempts to obtain the base of this earth in a separate state were related. They were made by passing potassium through red-hot silica. Potash was obtained mixed with a brown matter, which was converted into silica by the action of water. It would appear that silica contains nearly half its weight of oxygen; and Sir H. Davy conceives it to contain 2 atoms of oxygen to 1 of the base. This would make the weight of an atom of *silicon* (as we may denominate that base) to be 2, nearly or almost the same with that of sulphur. Sir H. Davy conceives that *silicon* is not a metal, but analogous to *boron* in its nature. These bodies possess intermediate properties between charcoal and sulphur.

3. The third part of the paper was on *chlorine.* A number of unsuccessful attempts to decompose this substance, and obtain oxygen from it, were stated. Sulphuret of lead was fused in it; but not the least trace of sulphate of lead could be obtained. Various experiments of a similar kind were tried, with an equally unsuccessful result. The author noticed the scepticism still entertained by many persons respecting the nature of chlorine, and the arguments brought forward in order to show that it might contain oxygen. Every candid person, he observed, who had seen the combination of dry muriatic acid gas and ammoniacal gas, must be convinced that no more water existed in the compound formed than had previously existed in the gases. If water were really formed, it would indicate rather the decomposition of azote than the existence of water as a constituent of muriatic acid. The objections of Berzelius from the doctrine of definite proportions are merely apparent, as the one doctrine can be reconciled to these proportions just as easily as the other. The author concluded his paper with some excellent observations on the mode of reasoning in chemistry. Lavoisier had the glory of first introducing sound logic into the science. Chemists may doubt whether there be such a thing as real elements, but they are not at liberty to doubt whether a substance has been decomposed or not, when all attempts to decompound it have failed. Oxygen has been considered as the acidifying prin-

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* See *Annals of Philosophy,* vol. ii. p. 48.

ciple; but hydrogen forms at least as many acids as oxygen, and it forms several into which oxygen does not enter at all. All cases of combustion were ascribed to the presence and agency of oxygen; but we now know that it takes place whenever bodies combine with energy; and fluorine, chlorine, and iodine, are entitled to the name of supporters, as well as oxygen. He suggests the possibility that the diamond may be a compound of charcoal and some very light unknown supporter.

At the same meeting a paper by Anthony Carlisle, Esq. *On Monstrosity in the Human Species*, was read. The author detailed a number of examples of monstrosity, hereditary in particular families, and propagated from one generation to another. All monstrosity he conceives to take place only in cases where the artificial civilization of mankind has interfered. Thus varieties of dogs, pidgeons, &c. are easily propagated.

On Thursday, the 10th of February, a paper by A. B. Brodie, Esq. on the influence of the nerves on the secretions of the stomach was read. The experiments consisted in cutting the gastric nerves of dogs, and giving them doses of arsenic sufficient to produce death in a few hours. This poison in common cases occasions a great secretion of mucus in the stomach and intestines. But in these experiments nothing was found in the stomach after death. Hence the nonsecretion of mucus seems owing to the section of the nerves.

At the same meeting a paper was read by Charles Kœnig, Esq. on the human skeleton from Guadaloupe, lately deposited in the British Museum. Mr. Kœnig introduced his paper by a historical sketch of all the facts that had been ascertained respecting fossil bones. Kamper, Blumenbach, and, above all, Cuvier, are the naturalists that have most distinguished themselves in these researches; but hitherto no human fossil bones had been discovered. Hence it was concluded, either that man was of subsequent creation to those animals, the fossil bones of which have been found, or that if human fossil bones exist they are covered by the existing ocean, and thus for ever concealed from our sight. The fossil human bones found at Guadaloupe appear to constitute an exception to this general rule. They were discovered by the French governor of Guadaloupe; and the specimen at present in the British Museum was dug up by him at considerable expense, and was intended for the museum at Paris. The capture of the island by Great Britain enabled Sir Alexander Cochrane to send it to the British Museum.

It was found near the sea shore in a calcareous rock of the hardest texture, being considerably superior in that respect to statuary marble. The rock is partly granular, and partly compact. The granular part is a mixture of grey and flesh-red particles. The red particles Mr. Kœnig considers as the millepora miliacea in fragments; it contains also a few shells. In short, it seems to consist chiefly of a congeries of fragments of corralines connected together firmly without any apparent cement.

The bones of the skeleton are not petrified, but retain the usual constituents of fresh bone; and when first exposed to the air were rather soft. The skull and vertebræ of the neck are wanting. The seven true ribs and three of the false ribs of the left side remain; but on the right side these bones are destroyed, though the sternal part of the true ribs adhere to those on the left side. The sternum is not visible, being probably sunk into the stone. The dorsal vertebræ are all visible, though not perfectly well defined. The forearm and finger bones of one hand remain, and one clavicle. The pelvis is pretty entire, and so are the thigh bones. The legs are so twisted in, that the fibula is sunk in the stone.

As to the age of this skeleton, there are no data to form a correct estimate, though in all probability it is not very recent. The appearance of the stone shows decidedly that it does not owe its origin to any calcareous deposition similar to calcareous tuff; but that its formation is analogous to that of common sandstone. It contains traces of phosphate of lime, which seems to demonstrate its animal origin.

On Thursday, the 17th of February, a paper by John Davy, Esq. *On Animal Heat*, was read. The author made a set of experiments in order to determine the specific heat of arterial and venous blood. He employed chiefly the blood of lambs. Two methods of experimenting were followed. 1. The relative times of cooling of equal bulks of arterial and venous blood were determined. The specific gravity of the venous blood was 1·050; that of the arterial, 1·047. This method gave the specific heat of arterial blood about 0·93; and that of venous blood, 0·92. 2. These two kinds of blood were mixed with water, and the change of temperature marked. The results differed somewhat in different experiments. Arterial blood by this mode of experimenting came out of the specific heat 0·95, venous blood 0·94 nearly. It appears to me that these experiments of Mr. Davy are liable to two objections, which must prevent us from putting full confidence in the results which he obtained. 1. It is probable that a chemical action takes place between blood and water; therefore the specific heat of blood cannot be accurately determined by mixing it with water. Suppose we were to mix alcohol and water: the temperature of the mixture would not enable us to determine the specific heat of alcohol. Neither, I am persuaded, would such a mixture enable us to determine the specific heat of blood. 2. Mr. Davy, in his experiments, often drew the venous blood of the animal on one day and the arterial on another. Now experiments of this kind never lead to accurate results. Whenever you begin to tamper with an animal you throw it into an unnatural state, and then it is impossible to calculate what sudden changes may be produced on its blood. Mr. Davy made a set of experiments to determine the temperature of arterial and venous blood in animals. The arterial blood was always hottest. In the sheep it was 104° or 105°, while the venous was

103° or 104°. In the ox it was 101°, while the venous was 100°. These results Mr. Davy considers as favourable to Dr. Black's theory of animal heat, and likewise to the notion that the heat depends upon the nervous energy.

At the same meeting a paper by Mr. Ivory was read, or rather announced, on the method of determining the orbit of a comet from three geocentric observations. As this paper was only partially read, it is impossible to give any account of it.

LINNÆAN SOCIETY.

On Tuesday, the 1st of February, a communication from Mr. Reynolds Johnson was read, giving an account of two fossil alcyonia found near Lyme. They were in flint, and in a very perfect state of preservation, and enabled Mr. Johnson to make several additions to our knowledge of the structure of this animal.

At the same meeting a paper was read by Mr. Roscoe on the class monandria. It consisted chiefly of strictures on Dr. Roxbourgh's essay on that class, in the eleventh volume of the Asiatic Researches.

On Tuesday, February 15, the conclusion of Mr. Roscoe's paper, entitled *Remarks on Dr. Roxburgh's Description of the Monandrous Plants of India*, published in the first volume of the Asiatic Researches, was read. Mr. Roscoe contends for the sufficiency of the generic characters derived from modifications of the anther-bearing filament of Seitamineæ, as proposed by him in his essay on this subject, published in the eighth volume of the Linnæan Transactions; and denies that Dr. Roxburgh's primary characters, taken from differences in the inner limb of the corolla, are of universal importance in this tribe of plants. He then proceeds to remark on certain species described by Dr. Roxburgh, whose *phrynium dichotomum* he regards as probably not specifically different from *marauta arundinacea.* His *phrynium virgatum* he is inclined to refer to *thalia geniculata.* Both Dr. Smith and Dr. Roxburgh have referred *amomum repens* of Sonnerat (the plant producing the lesser cardamoms of the shops) to the genus alpinia. Mr. Roscoe, however, is induced to agree with Dr. Maton, who in his observations on Mr. White's paper on cardamoms, published in the tenth volume of the Society's Transactions, has proposed to establish this plant as a new genus, under the name of *elettaria*; and on the authority of the published figures, Mr. Roscoe considers it as differing from alpinia, not only in habit, but in the structure and appendages of its anther-bearing filament.

With regard to Dr. Roxburgh's *globba radicalis*, he adopts the opinion of Dr. Sims, who in the Botanical Magazine (N° 1428) has established this as a distinct genus, under the name of mantisca saltatoria. Mr. Roscoe considers this genus as differing from globba no less in the structure of its filament than in its inflorescence.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute of France during the Year 1813.

I. Mathematical Department.—By M. le Chevalier Delambre, Perpetual Secretary.

Memoirs of M. Laplace on the Variations of the planetary Elements, and on the Comets.

The most interesting article of this notice of the labours of the scientific class ought to be the announcement of the *Mechanique Analytique* of M. le Comte Lagrange. The printing of the second volume had commenced about the beginning of January. The ardour with which the illustrious author devoted himself to this labour gave us the hope of soon reaping the fruits of it. His death, which at all times would have been a misfortune for the sciences, has been the more sensibly felt because it delayed, or perhaps destroyed altogether, the expectations which appeared so well founded; but the influence of a great man is not confined to the limited period of his existence. The domains which he has added to science will be cultivated and fertilized by his successors. In the success which they may obtain we shall see the result of his own happy labours. Thus when we read the memoirs with which Comte Laplace has enriched the collection of this year, we shall recollect that one of the first labours of M. Lagrange had for its object the theory which constitutes the foundation of the calculus of *probabilities*, which he afterwards treated in a manner so new and so remarkable. It will be recollected also, that in the last memoir which he read to the Institute he had simplified and generalized with the same superiority the calculus of the *variations of the elements of the planetary orbits*.

These are the two subjects to which M. Lagrange, who had likewise treated of them in his own way, has just added new developements. We shall satisfy ourselves at present with a simple announce of the last of his memoirs, which we merely heard once read over. We can give a more complete notion of the first memoir, which has been just published.

M. le Comte Laplace, who gave us last year a complete work on Probabilities, has applied his theory to one of the most difficult questions which physical astronomy presents: namely, the origin of comets, and the nature of their orbits.

Herschel's opinion on this subject will be recollected, who perceiving almost every where in the celestial spaces a matter feebly luminous, in which he observed disseminated certain points that appeared to him more dense and more luminous, conceived that in time universal attraction might unite round these centres the nebulous matter with which they are surrounded; that in consequence of their mutual attraction two or more of these centres might acquire a movement; that this motion might carry them to the surface of the sphere over which the attractive energy of the sun extends; and that this motion combined with the solar attraction might convert these centres into as many new comets, circulating round the sun, and following the same laws as the planets; that this may have been the origin of all the planets, and doubtless likewise of the sun and stars; for if we are obliged to admit the anterior existence of these great bodies, we may as well assign the same date to the much less important bodies that circulate round them.

The new orbits will be circular, or elliptic, or parabolic, or hyperbolic.

In the first case the comets will be always invisible, unless we suppose, against all probability, that their mass and inherent light are sufficient to render them visible at so great a distance; for these nuclei or centres perceived by Dr. Herschel are invisible to a common telescope.

If they are eliptical or parabolic, the comets may come so near the sun that they will become visible in a portion of their orbit comprehended between the perihelion and the parameter, and even a little beyond it. This supposition will explain sufficiently well the phenomena which the comets hitherto observed have presented. Their greater axis ought to go beyond the sphere of the sun's activity, which must extend much farther than the orbit of Uranus. Such elongated elipses must be sensibly confounded with parabolas having the same summit. The revolutions of such comets will be so long that we can scarcely expect to see them again, or to know them again, after all the alterations which they may have experienced in that part of their orbit where we cannot follow them, and in which so many causes may modify their elements.

This would likewise give a good explanation of the length and tenuity of their tails. The nebulous matter, condensed by attraction to form the comet, being dilated by the solar heat, would resume nearly its primitive tenuity, and may even evaporate and be lost in space. Having lost its tail and its nebulosity, the comet will be exactly similar to the four little planets. It may be even entirely dissipated, and cease to exist. Astronomers would have to regret the time lost in calculating its elements. It would be sufficient for them to be in possession of this coarse approximation, which puts it in their power to satisfy the public at an easy rate during the short time that the comet is visible from the earth.

But some comets have presented particularities inconsistent with this hypothesis. That of Halley, for example, has the great axis of its orbit smaller than that of Uranus. The comet of 1770 had a great axis less than that of Jupiter. According to M. Laplace these singularities may be produced by the planetary attractions, or the resistance of the ethereal medium; but the planetary attractions ought to be very weak at the entrance of the comet into the sphere of activity of the sun; and Laplace himself has rendered the resistance of the ether very problematic, and inconsistent with the constancy of the great axes of the planetary orbits.

If an elongated eliptical orbit may be confounded with a parabola, the difference is not greater between the parabola and hyperbola. The insufficiency of the parabola in some cases has been ascertained, and it has been found necessary to have recourse to the ellipse. Why has the necessity of the hyperbola never been felt? Of 117 comets of which we possess the elements, two only are decidedly eliptical; the 115 others are parabolic or eliptical. In fact there is nothing to prevent us from considering these orbits as hyperbolic, but differing infinitely little from the parabola, and in such a case there would be no reason for being surprised that the nature of these orbits had escaped us. The eliptical orbit of the comet of 1759 was only known by its different returns in an interval of 75 or 76 years. Had it not been for that we should have regarded the orbit as parabolic. The hyperbolic comets never return. Hence no opportunity can ever occur of rectifying our mistake. Hence we have no proof that the hyperbolic orbits are more rare than the eliptical. They may be even much more numerous without our ever suspecting it; but M. Laplace speaks of those only whose hyperbolic orbits may be recognised by observations. The fact is, that we do not know one of that kind.

Hence arises this question: What is the cause that the hyperbolic orbits are so rare? This problem cannot be completely solved. All that we can do is to apply to it the calculus of probabilities. If among several different cases which appear equally probable there be one which seldom or never occurs, we seem authorised to conclude that there exists a cause for its being so rare. The chances which give a hyperbola must be very few when compared to the others. M. Laplace finds, in fact, that *we may wager a great many to one that a nebulosity which penetrates within the sphere of the sun's activity, so as to be capable of being seen, will describe a very elongated elipse or a hyperbola which from the greatness of its axis will sensibly coincide with a parabola in the part observed.* It follows from the analysis of M. Laplace that in the case most favourable to hyperbolas it is 56 to 1 that the hyperbola will not be sensible. Thus we might be tempted to exclude this curve, and it would be so much calculation saved. In fact the hyperbola is never tried till both the parabola and elipse have failed.

The parabola itself is only a limit, a single case between the ellipse and hyperbola, the dimensions of which may vary to infinity. Hence there is scarcely any probability of a parabolic orbit. The hyperbolas are scarcely sensible, and from experience it appears that the ellipses are hardly more so. The hyperbolic comets never return, the elliptical not till after a long interval. We have no reason to be surprized that hitherto only one has returned constantly.

Laplace had found in the attraction of Jupiter a probable cause why the comet of 1770 has not appeared eight times since that period. Perhaps it is entirely evaporated, or reduced to a nucleus so small and so little luminous, that it will remain for ever invisible. These two explanations, far from being incompatible, may have concurred to produce an effect which has excited the curiosity of mathematicians and astronomers.

It may be dreaded that the new hypothesis will somewhat diminish the importance of the comets; and that it is similar to the opinion of the ancients, who considered comets as temporary collections of vapour, which were speedily dissipated and destroyed. Hence the astronomers long considered them as unworthy of being scrupulously observed. All the difference is, that Aristotle places the comets below the moon, while according to the hypothesis of Herschel, they are formed beyond the planetary system, and they last a much longer time; since it is only in their perihelion, that they are liable to lose a portion of their substance. But if they become invisible to us, that is the same thing, as far as we are concerned, as if they ceased to exist. Notwithstanding these reflections, which must be considered as mere conjecture, there is little doubt that the first comet which appears will be followed with as much zeal as ever; that intrepid calculators, not satisfied with the parabola, which is always sufficient for the first appearance, will endeavour to find an elipse, notwithstanding all the uncertainty under which such a problem labours, and that if neither of these curves will represent the orbit they will have recourse to the hyperbola, the very rarity of which will give it a new value.

(*To be continued.*)

ARTICLE XIV.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *New Geological Society.*

A Society has been established at Penzance under the name of the Cornwall Geological Society. Davies Giddy, Esq. M. P. is President. Among the members are Sir Christopher Hawkins, Sir John St. Aubyn, Rice Price, Edward Stackhouse, John Hawkins, William Rashley, William Carne, John Williams, Esquires, Much important geological information may be expected from a society of enlightened men, situated in the most interesting county in England in a geological point of view. Many curious facts discovered in the mines in that country have been lost for want of such a society.

II. *Coloured Halo round the Moon.*

ON Tuesday the 1st of February, about a quarter past eight in the evening, there was a very large and brilliant halo round the moon. It was by far the finest I ever saw. Immediately round the moon's disk there was a circular space which had the appearance of a white thin cloud. This was bounded by a bright red circle, passing insensibly into yellow. Beyond this there was a very broad circular band of very faint blue, which terminated the halo. About half past eight, the halo suddenly disappeared, and the sky round the moon became quite clear.

III. *Late extraordinary Fog.*

By an anonymous gentleman, in a letter which has on it the Richmond post mark, I have been favoured with the following circumstance respecting the extraordinary fog, mentioned in the last number of the *Annals of Philosophy*. "It is a very singular fact, that the thick rime which settled on the trees during the fog, from December 27, to January 2, was not disposed equally round the twigs and boughs, but was four times as deep on that side which was exposed to the east, as an any other. This is particularly remarkable, as there was no wind."

I did not myself make this remark at Chelsea, where most of my observations on the fog were made. Neither did I perceive any such difference in the trees in St. James's Park. Though the weather during the fog was calm, yet it is to be presumed, from the coldness of the temperature, that there was a tendency in the atmosphere to move from east to west. Accordingly, when it began to blow in the beginning of January, the wind was decidedly east. From the higher situation of Richmond than that of Chelsea, or St. James's Park, it is probable that the current would be more steady in the latter place, than the former. This imperceptible current will serve to account for the fact adduced by my correspondent, while at the same time it affords us a source for the great quantity of water deposited, which I was unable at first to account for. Let us suppose the current to have moved at the rate of one mile only in four hours, in which case it would have been imperceptible to us, then six atmospheres would have passed over our heads in one day, and 42 atmospheres during the week that the fog lasted. The possible deposition from such a quantity of air, would have been 28 ounces on every square foot of surface, which I think, is probably more than the quantity actually deposited.

IV. *Cold produced by the Evaporation of Sulphuret of Carbon.*

The following is an extract of a letter on this interesting subject, which I received from Mr. J. Murray, Philosophical Lecturer, about the end of January, and too late for insertion in the last Number of the *Annals of Philosophy.*

"Having been lately engaged in experiments on the oxidation of gold; I had rubbed leaf gold in a glass mortar with saliva, and upon adding a minute portion of the sulphuret of carbon, again began to triturate, when the whole became a concrete icy mass.

"A glass of water has remained on the table since the preceding evening, and though it might be some degrees below 32° Fahr. it indicated no disposition for congelation. A few drops of sulphuret of carbon were applied to the surface, instantly the globules became cased with a shell of icy spiculæ of retiform texture. Where they were in contact with the water, plumose branches darted from the sulphuret, as from a centre, to the bottom of the vessel, and the whole became solidified. The sulphuret of carbon in the interim volatilized, and during this period, the spicules exhibited the colours of the solar spectrum in beautiful array.

"If a single drop be suffered to float on the surface of a small volume of water, it will manifest an instant reduction of temperature, but I could not observe with those larger globules, which from their specific gravity sunk in the fluid, any alteration, provided there were none floating on the surface.

"I have been pleased, by suffering first the crust of ice to form around the lower surface of the floating sulphuret, and then to agitate the glass so as to dislodge the globule from its cell, when another pelicle forms around, and entwines it, which may be again displaced, and so on.

"I am investigating further the properties and effects of this curious substance, and should any particular phenomena occur, I shall feel happy in communicating the results to you."

V. *Arsenite of Silver.*

Arsenite of silver having been found the best means of detecting white arsenic when in solution, I think it will be worth while to state the most striking properties of this salt, as they were investigated by Dr. Marcet, and published by him in a short paper in the third volume of the Medico-chirurgical Transactions.

When this salt is first formed it has an orpiment yellow colour, but when allowed to stand for some time in an open vessel, it gradually becomes brown; and this is the colour which it has when dry.

It is perfectly insoluble in water, but dissolves readily in diluted nitric acid. If an excess of ammonia be added at the moment of its formation, it is dissolved, but when it has been once dried, it is no longer soluble in ammonia.

When heated in a glass tube a white smoke evaporates, which condenses on the sides of the tube in minute octahedral crystals of white arsenic, while a dark brown mass remains, obviously a subarsenite. Before the blow pipe upon a slip of platinum, and still more easily upon charcoal, the silver is reduced, and the arsenic totally dissipated.*

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* Dr. Roget, in publishing Dr. Marcet's remarks on this test, in the second volume of the Medico-Chirurg. Trans. p. 158, not only referred to Mr. Hume's letter on the subject, printed in the Phil. Mag. for 1805, but gave a summary account of the method therein proposed. Had I been aware of this circumstance, when I noticed Mr. Hume's reclamation of priority in the discovery of this test, so far as nitrate of silver is concerned, I should have observed that his anxiety on this occasion was quite superfluous, since Dr. Marcet and Dr. Roget, far from having overlooked Mr. Hume's paper, had, in the first instance, pointedly ascribed to that chemist all that belongs to him on the subject.

VI. *The late Storm.*

The late storm, being the most severe and the longest which has occurred in Great Britain since the year 1796, I conceive it will be worth while to state a few particulars respecting it, as it appeared in the vicinity of London, partly by way of record, and partly to enable my readers in other parts of the country to compare the storm in the neighbourhood of London, with its appearance in their own vicinity.

A severe frost had commenced on the 13th of December, and continued till the night of the 16th. The thermometer stood at 19° in the night between the 14th and 15th, and in the night between the 15th and 16th. On the 16th it thawed, and continued fresh (though it froze twice during the night) till the 26th, the day after Christmas. This was Sunday. It was clear and nearly calm, and the sun shone the whole day. On that day the frost set in at London, and continued, if we do not reckon an interval of four days, (the 27th, 28th, 29th, and 30th of January) for 42 days, or exactly six weeks. During the first week, the thick fog already described in the *Annals of Philosophy*, obscured the whole atmosphere, and rendered travelling dangerous. This fog did not go so far west as Sidmouth; but we know that it existed at Harwich. On Monday the 3d of January an east wind sprung up, which dissipated the fog. This wind continued without intermission till the 26th in the evening, when it changed to the south-west, and brought on a very gentle thaw. This thaw continued about four days, when the wind again shifted to the north, and blew steady and cold. The frost returned; though upon the whole the cold was not so intense, and the weather was much clearer, and there was even a good deal of sun-shine, which had not been the case before the thaw. This second frost continued till the 6th of February, when the wind shifted again to the south, and finally put an end to the storm.

A good deal of snow fell during this long continued frost, but in the vicinity of London the fall was not deep. At Chelsea and that neighbourhood, where I had an opportunity of making my observations, it was no where deeper than about a foot. Ice had formed upon the river at some distance above London, but the tide kept it clear for a long time between Battersea and the sea. Above Battersea the river appeared to me quite frozen over about the 20th of January The ice was covered with great quantities of snow, and large lumps of these floating down were driven backwards and forwards between the bridges. The thaw of the 26th of January brought down such prodigious quantities of this floating ice, that the whole river between Blackfriars and London bridge was filled with it; and when the succeeding frost came on, it bound together all these masses, so that the river was completely covered with ice, from London bridge up to Westminster, and might at one time have been crossed in almost any place.

The greatest cold at Chelsea was 5°: it happened on the night which followed the 9th of January. At Kensington I am told, the thermometer stood at 3°. The mean height of the thermometer before the thaw of the 26th was 25·572°. Its mean height during the day was 29·467°; and during the night 21·677°. The mean during the last week of the frost was 31·827°. The mean for the night was 26·555°, and for the day 37·111°. Finally, the mean during the four days of thaw was 34·875°. The mean for the day was 37°, for the night 32·75.

ARTICLE XV.

New Patents.

THOMAS WRIGHT, London, broker; for a method of making a composition or mixture for dying scarlet and other colours. Dec. 9, 1813.

JOHN BATEMAN, Wyke, York; for an improvement on musical instruments. Dec. 9, 1813.

JOSEPH WHITE, Leeds, millwright; for an improvement on steam-engines. Dec. 14, 1813.

JOHN SWARBRECK ROGERS, Chester, merchant; for a mode of spinning or making a species of wool into yarn, either by itself, or with any other material; which yarn may be beneficially used in various branches of manufacture. Dec. 14, 1813.

ARTICLE XVI.

Scientific Books in hand, or in the Press.

Mr. Hodgson is about to publish a Treatise on Aneurisms and wounded Arteries, in an octavo volume, with Engravings.

Mr. Stewart will shortly publish a Treatise on Uterine Hemorrhage.

Mr. William Goodlad, of Bury, has in the press a Practical Essay on the Diseases of the Vessels and Glands of the Absorbent System; with an Appendix, containing Surgical Cases and Remarks.

A Translation of the Treatise on Mechanics, which forms the Introduction to the Mechanique Celeste of Laplace, with Explanatory Notes, &c. by the Rev. John Toplis, is preparing for publication.

Dr. Adams has in the Press his long projected work on the erroneous opinions and unfounded terrors usually entertained concerning Hereditary Diseases. Connected with the subject are some Remarks on Cutaneous Diseases, on the attempts at reducing them to Orders and Classes, and on the unnecessary revival of obsolete Greek Terms.

ARTICLE XVII.

METEOROLOGICAL TABLE.

1814.	Wind.	BAROMETER.			THERMOMETER.			Min. on the Snow.	Snow &c.	
		Max.	Min.	Med.	Max.	Min.	Med.			
1st Mo.										
Jan. 13	N E	30·13	30·03	30·080	30	14	22·0	8		☾
14	N E	30·03	29·61	29·820	26	19	22·5	14		
15	E	29·61	29·30	29·455	31	20	25·5	21		
16	N E	29·63	29·25	29·440	32	22	27·0	19	—	
17	N	29·25	29·14	29·195	30	11	20·5	8	—	
18	E	29·14	29·07	29·105	36	30	33·0		—	
19	N E	29·42	29·07	29·245	34	28	31·0		—	
20	N E	29·76	29·42	29·590	33	14	23·5	6	—	
21	Var.	29·76	29·72	29·740	25	14	19·5	11	—	●
22	N	29·82	29·71	29·765	32	8	20·0	6	—	
23	N	29·77	29·69	29·730	35	15	25·0	11	—	
24	Var.	29·88	29·77	29·825	33	24	29·5		1·25	
25	Var.	29·88	29·60	29·740	36	20	28·0		—	
26	S W	29·60	29·22	29·410	36	33	34·5		·12	
27	W	29·16	29·11	29·135	39	33	36·0		·20	
28	Var.	29·11	28·54	28·825	40	28	34·0		—	
29	Var.	28·94	28·22	28·580	41	32	36·5		·86	☾
30	W	29·32	28·94	29·130	40	25	32·5	20		
31	N W	29·80	29·32	29·560	38	26	32·0		—	
2d Mo.										
Feb. 1	N W	30·01	29·90	29·955	36	26	31·0		6	
2	N	30·04	30·02	30·030	41	24	32·5	22		
3	N	30·01	29·98	29·995	33	19	26·0	2		
4	W	30·15	30·11	30·130	32	19	25·5	12		○
5	S W	30·11	29·68	29·895	38	29	33·5		8	
6	N W	29·61	29·51	29·560	44	33	38·5		4	
7	W	29·65	29·48	29·565	40	32	36·0		—	
8	S W	29·70	29·36	29·530	50	35	42·5		5	
9	S W	29·94	29·70	29·820	47	40	43·5			
10	S W	29·98	29·94	29·960	49	42	45·5			
11	S	29·95	29·90	29·925	50	35	42·5			
		30·15	28·22	29·591	50	8	31·31		2·66	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

First Month.—13. Much wind last night: very fine day: *Cumulus* and *Cirrostratus.* 14. a.m. Somewhat cloudy. 15. Overcast with *Cirrostratus:* light breeze. There being no evaporation to-day, the surface of the snow is a little warmer than the air. 16. Overcast: a slight thaw, from the warmth of the earth: at evening snow and frost again. 17. A clear day: *Cirrus* and *Cirrostratus* in the evening, with a low *Nimbus*, or two, forming. At sun-set, the temp. being 18°, there was a bed of haze in the E. like that which accompanies the fall of *dew*, but much more dense, and singularly coloured. It was chiefly of an indigo blue, but passed below into opake white, and above into a faint red, more transparent. 18. A snowy morning: small snow and sleet through the day. 19. A snowy day. 20. Snow: the wind strong at N.E. 21. At 8 a.m. beginning to snow again. In the drifts the snow is many feet deep. This day proved fine and calm: the wind, at times, S.W. 22. A little snow in the morning: fine day: strong breeze, with various clouds. 23. Snow, morning and evening. There was a fine exhibition of clouds: as *Cumuli*, well formed, but with little colour; the superior modifications, including the *Cirrocumulus*; and several distinct *Nimbi*. 24. The sky as yesterday: snow, a.m. the wind brisk: about two p.m. a squall, with plenty of snow. 25. a.m. A fine elevated grey sky: then, *Cirrus* and *Cirrostratus*, the wind veering by N. to W. It has had lately much tendency to this quarter at night. 26. A hollow wind at S.W.: snow, followed by small rain. 27. A misty thaw: a little rain at intervals: wind and rain in the night. 28. a.m. Misty, with rain: the wind W.: after this a little snow: then fair, and frosty after sun-set. 29. Stormy: the wind S.E. with snow early: then steady rain, followed by more snow. The *minimum* of pressure occurred after six p.m. It was not confined to a small space of time. As the barometer began to rise, the wind came round by S.W. to N.W. and blew with great violence till near morning. 30. A fine day: strong breeze, with clouds: at sun-set a sprinkling of opake hail. 31. Fair day: but at seven p.m. a sudden heavy snow shower.

Second Month.—2. Hoar frost. 3. Hoar frost. A very fine sunny day, followed by bright moonlight. A thermometer, at an intermediate height, between the standard one and that on the snow, gave 14° as the minimum. The latter instrument at eight a.m. had risen only to 4°, and was thickly covered with *rime*, although at night there had been an appearance of strong evaporation. 4. Sunshine, and fair night. About nine p.m. a colourless halo, of the largest diameter, with *Cirrus* clouds. At half past ten a small coloured halo, with *Cirrostratus*. The temp. on the snow being 12°, the wind W.—a breeze, I now exposed, in a metallic dish, close to the thermometer, 2600 grains of snow (which had become hard by freezing) in two or three masses. At half past eight, or in ten hours, the temp. having risen to 28°, it had lost 27 grains in weight. 5. Crimson sky at sun-rise: hollow wind: snow and sleet. 6. A gale from S.W. with showers of rain: at evening it cleared up, and blew from N.W. 7 to 11. *Cirrostratus* and *Cirrocumulus*: red sky at sunrise: gales of wind, and a few showers.

RESULTS.

Winds, Northerly in the fore part of the period, variable in the middle, and in the conclusion Southerly.

Barometer: greatest height ....30·15 inches;
Least ....28·22 inches;
Mean of the period ....29·591 inches.

Thermometer: Greatest height.................50°
Least .... 8°
On the surface of the snow ..... 2°
Mean of the period............31·31°

Product of the rain-guage ....2·66 inch.

Evaporation (from water containing a little salt) ....0·32 inch.

Some observations on the present winter, compared with the last severe one (of 1794—5), on the origin of the fogs which prevailed in each, and on the recent great depression of the barometer, must be deferred to next communication.

TOTTENHAM, *Second Month*, 19, 1814. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

APRIL, 1814.

ARTICLE I.

Biographical Account of M. Malus. By M. le Chevalier Delambre.*

ETIENNE-LOUIS MALUS was born at Paris, on the 23d of July, 1775. He was the son of Anne-Louis Malus, of Mitry, and of Louise-Nicole-Charlotte Desboves.

The first education, which he received in the house of his parents, was principally directed towards literature; and he had made so much progress that, even to the last, he could repeat by heart long passages out of the Iliad. At the age of seventeen or eighteen he wrote a tragedy in five acts, entitled, The Death of Cato: but this did not prevent him from devoting a considerable part of his time to very different studies; since at that period he underwent a successful examination, in consequence of which he was admitted into the *Ecole du Genie.*

After having distinguished himself there by his inclination for analysis, it was his turn to be appointed an officer of *genie militaire:* but he was rejected as a *suspected person* by the Minister Bouchotte; and this kind of civil interdiction, depriving him of all hopes of advancement, he repaired to the army of the North, was incorporated in the 15th battalion of Paris, and was employed as a common soldier in repairing the harbour of Dunkirk. The officer of *genius* who presided over this undertaking did not fail to notice him, and to perceive how much he was misplaced. On his recommendation, Malus was recalled by the Government, and sent to the Polytechnic School; where he was soon after employed in giving a course of analytical mathematics, in the absence of M. Monge. Restored to the rank he had held at the period of his first nomination, he was almost immediately raised to that of Captain, and was employed as Professor of Mathematics in the School of Metz.

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* Translated from the Moniteur of the 16th January, 1814.

It was at this epoch (1797) that his military career began. He was present in the army of the Sambre-and-Meuse at the crossing of the Rhine, and at the battles of Ukrath and Alterkirk. The same year was marked by a more agreeable circumstance, which afterwards constituted the happiness of his life: it was then that he saw for the first time Madame Malus (Wilkelmine Louise Koch, daughter of the Chancellor of the University of Giessen, in the dutchy of Hesse-Darmstadt). Honour and duty prevented him at that time from realizing the wish which was dearest to his heart. He was obliged to embark for Egypt, assisted at the battles of Chebreis and of the Pyramids, and at that of Sœbisch; and he was named a member of the Institute at Cairo: but his life was too active, and he was too much employed, to be able to devote himself to the sciences. A single opportunity presented itself, of which he profited with sufficient address. In a *reconnaissance*, in which he was employed with M. Lefevre, engineer of bridges and causeways, he was lucky enough to discover a branch of the Nile unknown before that time to travellers, to give a description of it, and construct a chart of a country into which no Frenchman had penetrated since the time of the crusades. (The memoir which he wrote on this subject makes a part of the first volume of the Decade Egyptienne.) But during the course of this memorable expedition, he chiefly distinguished himself as a military engineer.

Dangers of every kind attended him in Syria, at the siege of El Harisch, and at the siege of Jaffa, where he performed the duties of an engineer. After the taking of this last town, he was employed in repairing its fortifications, and establishing military hospitals in it. Here he caught the plague; but had the good fortune to recover without any assistance whatever. He had scarcely regained his health, when he was obliged to repair to Damietta to superintend similar labours. Thence he marched against the Turks who had disembarked at Lesbeh. He was present at the battles of Heliopolis and Coraim, and at the siege of Cairo. He then went to build Benisouef-Saioum, a fort intended to preserve the communication between the Delta and Upper Egypt. When he returned to Cairo he contributed to fortify that city against three great armies that were marching against it. Finally, he embarked at Aboukir, in the British transport The Castor, and arrived in the roads of Marseilles on the 14th of October, 1801, and disembarked at the Lazaretto of that city on the 26th of the same month.

Thus, exhausted by so many fatigues, and by the terrible diseases which had ruined his health for ever, he did not forget the engagement which he had formed four years before. His first care was to go in search of her who had received that promise, and who had shown no less fidelity, though in all probability she expected never to see him more. He married her, carried her to France, and received from her to his very last moments the most tender and heroic attention. She constituted the whole of his happiness, and was not able to survive him. (She fell a victim to the fatigues which had been occasioned by her care of him, and died on the 18th of August, 1813.) It was at the commencement of this happy union that M. Malus made himself known to the French Institute, by a work in which he treated, in the most general and rigorous manner, all the questions in optics that depend upon geometry alone. He there explained and calculated all the phenomena of reflection and refraction, and followed through all its changes the motion of the rays of light. This production drew the attention of philosophers to a phenomenon which had occupied Newton and Huygens, I mean double refraction. The Institute conceived the hope of seeing this remarkable phenomenon, which had employed the greatest geniuses without a satisfactory explanation, better understood. It was made the subject of a prize. This prize was gained by M. Malus, who showed that, besides mathematical knowledge, which he had displayed in his first work, he possessed the patience, the address, and the sagacity, which constitute a great philosopher. By delicate experiments he discovered in light remarkable properties either wholly unknown before, or which had not been exhibited in so clear a point of view. He discovered that resemblance of the molecules of light to the magnet which gives them poles and a determinate direction.

This success caused him to be elected a member of the Institute. He succeeded a philosopher whose name has been immortalized by a brilliant discovery (Montgolfier). In 1804 he was made a member of the Legion of Honour, and subdirector of the fortifications of Antwerp. In 1809 he was made subdirector of barracks in the department of the Seine: in 1810, member of the Committee of Fortifications, and major of *genius*. In 1811 he was second commander and director of the studies of the Polytechnic School, in which for some years he had fulfilled, to the satisfaction of the superiors and of the pupils, the severe duties of examiner. His different occupations did not prevent him from continuing those fine experiments upon which his reputation is founded, and for which he had received the gold medal given every other year by the Royal Society of London to the philosopher who has made the most striking discovery respecting *light* or *heat*.

The activity of Malus enabled him to discharge his duty in all these different employments. Though he still carried about him the seeds of that cruel disease which was so soon to deprive the scientific world of his assistance, he scarcely permitted a month, or even a week, to pass without laying before the Institute the new fruits of his researches. When his health no longer permitted him to attend the meetings of that body, one of his friends still continued to relate the result of his labours: but his disease made such rapid progress that scarcely was his illness known before it became certain that he could not recover. He was afflicted with continual pains, without ever making the slightest complaint, or testifying the smallest sign of impatience. Even when enfeebled by a long want of sleep, and incapable of all application, he deceived himself respecting the state of his health. He spoke of new arrangements, which would be required in consequence of his having been appointed to the place of director of the studies, a situation which at first he had only filled *ad interim*, and occupied himself in contriving new plans for the future, when his health should be restored. Did he wish to spare the sensibility of his wife, and of some friends, who never quitted him during the most painful period of his disease? No; he deceived himself. Had it not been for this mistake, which all around him made a point of encouraging, would he not have endeavoured to remove his wife from him? She never left him, even for an instant; and during five days and nights she remained with her face constantly fixed on his, ready to satisfy all his wishes. Would he not have been afraid of the effects of contagion? Would he have accepted of a care which, without being of any real utility to him, might prove, as was in fact the case, fatal to her who bestowed it? I wish I could here transcribe a letter which one of his faithful friends wrote just after the catastrophe that terminated this scene of grief! Let us rather dismiss these dismal ideas, and speak of the name which Malus has left behind him. His name is for ever attached to the phenomena of *polarized light*, of which he was the first person that spoke. All the discoveries relative to this branch of optics will recall the memory of the philosopher who first laid open this new path. Newton, in speaking of a young friend whom he had just lost, said, *If Cotes had lived we should have known something.* We may say the same thing. If Malus had lived he would have completed the theory of light. He died on the 24th of February, 1812. His place in the Institute has been filled by M. Poisson.

ARTICLE II.

Essay on the Cause of Chemical Proportions, and on some Circumstances relating to them: together with a short and easy Method of expressing them. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

(*Continued from p.* 106.)

4. *Wolfranium, tungsten.* (W).—Messrs. D'Elhuiarts, as well as Mr. Bucholz, found that 100 parts of this metal combine with 24 or 25 parts of oxygen, in order to form the yellow oxide or tungstic acid; and Mr. Aikin found 16 parts of oxygen for 100 of metal. The following are some experiments which I made with this metal.

After having in vain endeavoured to form sulphuret of tungsten by distilling a mixture of acid and sulphur, I mixed tungstic acid with sulphuret of mercury, and heated the mixture in a glass retort. But the retort was not capable of enduring the heat necessary to separate the sulphuret of mercury from the sulphuret of tungsten, as the two appeared to be chemically combined. I at last succeeded in forming sulphuret of tungsten by the following process. I mixed yellow oxide of tungsten (obtained from crystallized tungstate of ammonia) with four times its weight of very pure sulphuret of mercury, in a Hessian crucible. I covered the surface of the mixture with charcoal. This crucible I inclosed in a larger one, surrounding it with charcoal in a coarse powder. Over the whole I placed a cover, which did not prevent the escape of gaseous matter. I exposed this crucible to the greatest heat which I could raise in an ordinary furnace, for half an hour. I then allowed it to cool.

The sulphuret of tungsten thus obtained is a greyish black powder, which, when rubbed upon a polished hematite, assumes a beautiful metallic lustre. Under the hammer it concretes into metallic masses having some coherence. The metallic surface of this sulphuret has the colour of sulphuret of copper, but it is somewhat more blue. *a.* 100 parts of this sulphuret exposed to heat in a platinum cup, till it disengaged no more sulphurous acid gas, left for residue 93·5 parts of a brown oxide in the state of powder, which when exposed to a strong heat, became dark green without any change in its weight. *b.* 100 parts of the same sulphuret heated with nitromuriatic acid, produced with muriate of barytes 182 parts of sulphate of barytes. Hence the sulphuret of tungsten is composed of

Tungsten	75·04	100·00
Sulphur	24·96	33·26
	100·00	

But we have seen that this sulphuret produced by combustion 93·5 of tungstic acid; that is to say, that 24·96 of sulphur are replaced by 18·46 of oxygen. Hence it follows, that 100 of tungsten should combine with 24·6 of oxygen, to become tungstic acid.

We find here as well as with molybdenum, that the composition of the sulphuret is not analogous to that of the acid; but to an oxide whose oxygen is to that of the acid as 1 to 1½. If according to these data we calculate the composition of the acid from that of the sulphuret, we find that 100 of tungsten ought to combine with 24·9 of oxygen. But as the analysis of the acid is founded on that of the sulphuret, it is evident that the true point ought to be between 24·6 and 24·9. Till we get more exact experiments, I shall consider 24·75 as the correct number. Hence tungstic acid is composed of

Tungsten	80·16	100
Oxygen	19·84	24.75
	100·00	

To discover the oxide of tungsten whose composition is proportional to that of the sulphuret, I put a portion of tungstic acid into a glass tube, which I heated to redness in a small furnace, while a current of hydrogen gas passed through the red hot acid. The gas at first disappeared and produced vapours of water; but at last passed through the tube without alteration. I continued the current of hydrogen while the tube was cooling. I obtained the acid converted into a flea brown oxide, very inflammable, taking fire at a temperature considerably under a red heat, and burning like tinder, leaving for residue yellowish green tungstic acid. 100 parts of this oxide burnt upon a small plate of platinum, produced 107 parts of acid of tungsten. But these 107 parts contain 21·16 of oxygen, of which 7 is almost exactly the third part. Hence this oxide is composed of

Tungsten	85·84	100·0
Oxygen	14·16	16·5
	100·00	

This oxide is neither soluble in acids nor alkalies. It remains to be examined, whether it would not unite with acids at the instant of its formation; as for example, by heating powder of tungsten in muriatic acid gas. I ought to observe that Mr. Bucholz, (*Neues Journal fur Chemie und Physik von Schweigger*, t. 3. p. 15,) makes mention of a brown oxide of tungsten, obtained by the decomposition of tungstate of ammonia, which he considers as intermediate between the blue oxide and the acid. We shall see immediately that these two last bodies constitute in fact one and the same substance.

To determine the number of volumes of oxygen in tungstic acid, I examined tungstate of ammonia. This salt was composed with tungstic acid, treated with nitric acid, and then exposed to a red heat. Ammonia dissolves tungstic acid slowly, but the combination obtained is very pure. I put 10 parts of tungstate of ammonia dried and in powder, into a retort exactly weighed, to which I adapted a tubulated receiver filled with caustic potash. The receiver had a tube attached to it likewise filled with caustic potash. I heated the retort to redness, and kept it in that state till the disengagement of ammonia was over. There remained in the crucible 8·88 parts of an indigo blue powder, exceedingly beautiful. The receiver and tube being heated a little to drive off the remains of the ammonia, had gained 0·557 in weight, and the loss of weight occasioned by the escape of the ammonia amounted to 0·563 parts. This experiment was repeated several times, and the weight of the residue in the retort was 86·9; 87; 87·8; 88·8 in different experiments. But the first experiment was made with the greatest care. According to it the tungstate of ammonia is composed of

Tungstic acid	88·80
Ammonia	5·63
Water	5·57
	100·00

The 5·57 of water contain 4·91 of oxygen, and the 5·63 of ammonia contain 2·48. But 2·48 + 2 = 4·96. There are therefore two portions of water for one of ammonia, just as is the case in the sulphate, borate, and oxalate of ammonia. 100 parts of acid are combined with 6·34 of ammonia, which contain 2·914 of oxygen. Now 2·914 × 6 = 17·484. We have seen that the acid contains 19·8. Therefore this analysis shows us that it must contain 6 times as much oxygen as the base. And if we take the mean of the results obtained; that is to say, if we consider it as most probable that the tungstate contains 87·8 of tungstic acid, 100 parts of that acid are neutralized by 7 parts of ammonia, which contains 3·206 of oxygen. And 3·206 × 6 = 19.236.

I have stated that the residue in the retort after the decomposition of the tungstate was a blue powder. I considered it at first as containing less oxygen than the acid; but having treated 4 parts of this oxide with smoking nitric acid without producing any alteration on it, I exposed it to heat in a platinum crucible exactly weighed. It changed colour the instant it began to get red hot, and became straw yellow; and I found that it had neither gained nor lost weight. I repeated the experiment several times, and always with the same result. On the other hand, when I heated to redness the blue oxide in the retort in which it was formed, it did not change its colour; but if air was admitted, the portions that came in contact with it assumed a yellow colour. The blue powder dissolves in ammonia and in caustic potash, though more slowly than the yellow oxide. The solution is colourless, and I could not find that it contained any thing else than tungstic acid. But what is the difference between these two states of tungstic acid? How does the air contribute to change the blue colour into yellow? When the yellow acid is very strongly heated it becomes green, and at last blueish green. But it does not again recover its yellow colour when exposed to a less temperature.

100 parts of nitrate of lead were dissolved in water, and precipitated by a portion of the same tungstate of ammonia which had been found to contain 88·8 per cent. of tungstic acid. I obtained 235·5 parts of tungstate of lead. It constituted a yellowish mass similar to pure tungstic acid. This salt is of course a compound of

Tungstic acid	71·42	100
Oxide of lead	28·58	40
	100·00	

But these 40 of oxide of lead contain 2·86 of oxygen. This coincides well with the analysis of tungstate of ammonia, and shows us that if the result is not quite exact, this is owing to a fixed matter mixed with the acid, perhaps potash.

When we ask how many volumes of oxygen are in tungstic acid we hesitate between 3 and 6. The analogy of the arsenic and chromic acids, together with the circumstance that tungstate of ammonia cannot be united with more ammonia; and that it crystallizes such as I have described it, in a liquid containing a great excess of ammonia, shows us that tungstic acid must contain 6 volumes of oxygen. This corresponds likewise with the great specific gravity of this metal. The volume of tungsten then weighs 2424·24. The brown oxide is $W + 4\,O$, and the acid $W + 6\,O$.

5. *Stibium, Antimony.* (Sb). I have already in a preceding dissertation, described my experiments on this metal. I have pointed out the difficulties which prevented me from obtaining an exact result. It is only in consequence of experiments repeated according to the ideas which I have explained in this paper, that I consider myself as having gained more decisive results. I had found that 100 parts of this metal combine with 57·3 of sulphur, and that this sulphuret dissolves in concentrated muriatic acid, and forms muriate of antimony (murias stibiosus) and sulphureted hydrogen, without any excess either of sulphur or hydrogen. Hence it follows, that the oxide of antimony (oxidum stibiosum) ought to be composed of 100 metal and 18·6 of oxygen. The next oxide, according to the notions which I entertained at that time, contained 1½ as much oxygen, that is to say, 27·9. But in none of my experiments on the composition of antimonious acid, could I find that the metal absorbed that quantity of oxygen. This I accounted for by an imperfect oxydation, for which opinion I gave reasons in the dissertation already alluded to. But when I consider the ratio of the oxygen in the acids in *ous* to that in the acids in *ic*, which is generally as 2 : 3; but never as 3 : 4, I think that I endeavoured in these experiments to obtain a result which could not take place.

I resumed therefore my experiments on the antimonious acid, and I found that when pure antimony is oxidated in a phial by nitric acid, the mixture evaporated to dryness in a platinum crucible, and then heated till it becomes perfectly white, we obtain always the same results. I found that 100 parts of metal treated in this way produce always very nearly 124·8 of antimonious acid. In my former experiments I obtained a somewhat greater result, because I had always employed glass phials, which could not be exposed to a heat sufficiently strong to reduce the whole yellow oxide to white oxide. It follows then, that antimonious acid is composed of

Antimony	80·129	100·0
Oxygen	19·871	24·8
	100·000	

Now 18·6 : 24·8 :: 3 : 4; that is to say, that the oxide is composed of $Sb + 3 O$, and the acid of $Sb + 4 O$. This agrees very well with the capacity for saturation of this acid. Since I found that it contains 4 times as much oxygen as the base by which it is neutralized. For 100 parts of antimonious acid are neutralized by 30·5 of potash.

From analogy antimonic acid ought to be composed of $Sb + 6 O$. But it is scarcely possible to determine this point by the quantity of base with which the acid is neutralized. For if we suppose that the acid, contrary to all experience, is $Sb + 5 O$, the difference of oxygen in the base, in either case, would be only 4·52, or 4·73 at most. Now it is very difficult to make analyses of the antimoniates which do not vary more than this. When I oxidated the metal, I never was able to obtain more than 131 of yellow oxide from 100 of metal. In some cases I got no more than 128·5, or 129. All these oxides may be nothing else than combinations of the antimonious and antimonic acids, just as we have similar combinations between nitrous and nitric acids. I thought it likely that a more rigid examination of the hydrous antimonic acid would throw some light on the subject.

I prepared this hydrate in the following manner. I dissolved antimony in nitromuriatic acid, evaporated the solution nearly to dryness, and then added water. When the mixture had become clear, I decanted off the acid liquor. The white powder was then dried. To deprive this powder of all nitric and muriatic acids, from which it is not easily freed, I poured water upon and dried it a number of times in succession, till it had lost its acid and metallic taste. I introduced this white powder well dried into a glass retort, furnished with a tubulated receiver, into the tubular of which I had put muriate of lime. I then heated the retort red hot. There condensed in the receiver and in the tube 5 per cent. of pure water. The powder which was still yellowish, was strongly heated in a platinum crucible, and left for residue 91·18 of antimonious acid, of a very white colour. The acid then had lost 3·82 per cent. of oxygen. I repeated this experiment, but the results were always different. The only constant circumstance was the ratio of the water to the antimonious acid, which remained after the ignition of the mass. This ratio was such, that the acid always contained four times as much oxygen as the water. In the experiment which I have related, the oxygen of the water is 4·412, and that of the antimonious acid 18·1. Now 4·412 × 4 = 17·648. All that I learned by these experiments is, that it is not in our power to produce pure antimonic acid, nor an antimoniate which contains no antimonite of the same base.

These experiments then do not determine any thing respecting the composition of antimonic acid, and though they seem to agree best with the notion that it consists of Sb + 5 O, I think analogy so strongly in our favour, that we cannot avoid adopting Sb + 6 O, as its true composition. Especially as at present we do not know of a single example of a radicle combined with 5 volumes of oxygen.

If we calculate the volume of antimony from the composition of antimonious acid, it must weigh 1613. The oxides of this metal, from what I have said, ought to be: 1. The suboxide, Sb + O? 2. Antimonious oxide, Sb + 3 O. 3. Antimonious acid, Sb + 4 O. 4. Antimonic acid, Sb + 6 O. In my former experiments, I found that 100 parts of antimonious acid mixed with antimony in powder and exposed to heat, oxidize about $\frac{1}{3}$ as much metal as the acid contains, producing a fusible oxide, which I considered as a combination of antimonious oxide and acid, because its properties were different from those of pure oxide. But I have found that the difference was occasioned by a small quantity of silica and potash, which the fused oxide had dissolved from the glass in which the experiment was made.

6. *Tellurium.* (Te).—Tellurium has no other known oxide but that which is formed by the action of nitric acid. This oxide has the remarkable property of combining with acids as a base, and with bases as an acid. In these last combinations, which I call tellurates, the oxide of tellurium contains twice as much oxygen as the base. Hence I conclude, that it contains two volumes of oxygen. And as 100 parts of tellurium in my experiments produced 124·8 of oxide, the volume of this metal ought to weigh 806·48. If on the other hand, we take as the base of our calculation the analyses of tellurate of lead, (in which 201·6 parts of fused tellurate produced 157 of sulphate of lead,) it follows that 100 tellurium combine with 24·4 of oxygen, and that the volume of the metal weighs 819. But if the specific gravity be of any value in such determinations, it follows that tellurium ought to have the same weight as antimony, because their specific gravities differ very little. Now if we suppose oxide of tellurium to be Te + 4 O, its volume would weigh from 1613 to 1638. Future experiments must decide this point.

Tellurium is found in nature combined with different metals, and it has the property of uniting with hydrogen. Tellureted hydrogen, according to my experiments, is H + Te. The metallic tellurets contain, according to the analysis of Klaproth, 2 Te, and some of them 4 Te.

7. *Columbium.* (Cb).—We cannot calculate the volume of this metal, because we do not know the proportions in which it enters into any of its combinations,

8. *Titanium.* (Ti).—Richter found, (Neue Gegenstande Cah. 10, p. 120,) that a solution of muriate of titanium, which contained 84·4 of oxide of titanium, gave 150 parts of muriate of silver; and though we cannot put much confidence in the accuracy of this chemist, I thought it worth while to notice the experiment, as furnishing at least an approximation. According to it, 100 muriatic acid combine with 295·2 of oxide of titanium; that is to say, that the white oxide of titanium contains very nearly 10 per cent. of oxygen. But if the copper coloured oxide be Ti + O, the white oxide ought to be Ti + 2 O, and the volume of titanium ought to weigh 1801. I must observe however, that Vauquelin lays it down as proved by his experiments, that the white oxide contains 90 red oxide and 10 oxygen. But there is reason to conclude, that his white oxide contained potash.

9. *Zirconium.* (Zr).—Unknown.

10. *Silicium.* (Si).—In my experiments to reduce silica by means of iron and charcoal, I found that when the alloy of iron and silicium dissolved in muriatic acid, the silicium combines with a great quantity of oxygen. And after having determined the quantity of red oxide of iron, of carbon, and of silica produced by the decomposition of the alloy, I considered myself as entitled to conclude, that silica contains from 45·34 to 47·75 per cent of oxygen. Mr. Stromeyer, who has repeated these experiments with a good deal of care, has found by an analytical method different from mine, that silica must contain as much as 55 per cent. of oxygen. It appears that the best way of determining the composition of this earth would be to calculate it from the fluate of silica; but the composition of this acid being only known from that fluate, we cannot employ this method here. I have already, when treating of fluoric radiole, made observations on this subject, and have cited the ingenious experiments of Mr. John Davy, on several combinations of fluoric acid. Among these experiments, there is one which may be of use to us here: I mean the analysis of triple fluate of silica and ammonia. According to Mr. John Davy, it is composed of 24·5 ammonia, 46·357 silica, and 29·143 fluoric acid. Now it is necessary, that the oxygen of the ammonia, which is here the smallest quantity, should exist in the silica, multiplied by a whole number. But 24·5 of ammonia contain 11·219 of oxygen, and the silica (supposing it to contain 48 per cent. of oxygen), contains in 46·357 parts, 22·35 of oxygen. But 11·219 × 2 = 22·438. From this it would appear, that the result of my experiments is not very far from the truth, while it is impossible for silica to contain so much as 55 per cent. of oxygen, unless the experiments of Mr. Davy be very inaccurate, which I have no reason to believe. Another manner of verifying the composition of silica is to examine, with more care than is usually done, the composition of the minerals of which it constitutes an ingredient, and in which it must be combined with the other constituents, according to the laws of chemical proportions. We

know that the analysis of ytterite, made by the late Mr. Ekeberg, is one of the most exact that mineralogy possesses. In it we find 23 parts of silica combined with 55·5 of yttria and 16·5 of oxide of iron, such as analysis gives it, and equivalent to 15·42 of pure black oxide of iron. From experiments which I shall mention hereafter, 55·5 yttria contain 10·3 of oxygen; the 23 of silica, from the preceding data, should contain 10·9 of oxygen, and the 15·42 of black oxide of iron contain 3·5; now 3·5 × 3 = 10·5. This coincidence is an additional proof, that my determination of the composition of silica is very near the truth. I have not spoken of the 4 parts of glucina contained in ytterite, because I do not know the composition of that earth. But it is probable that they will not form an exception to the general rule.

The great quantity of oxygen in silica renders it probable, that it contains more than one volume, and as the composition of the triple fluate of silica and ammonia, shows that it cannot contain 3 volumes, it is probable that it contains 2. In that case the volume of silicium ought to weigh 216.

11. *Osmium.* (Os).—Unknown.

12. *Iridium.* (I).—Unknown.

13. *Rhodium.* (R).—Dr. Wollaston, to whom we are indebted for our knowledge of the existence of this metal, has been so good as to furnish me with the quantity necessary for determining the capacity of saturation of this rare, and difficult to be procured, metal.

My first essay was to reduce it from its triple muriate by means of mercury, in order to find in that manner the quantity of oxygen which it contained, by means of the quantity of mercury necessary to reduce it. But this experiment did not succeed. Rhodium cannot be reduced in that manner. We obtain a black powder composed of amalgam of rhodium, muriate of mercury (calomel), and an insoluble muriate of rhodium, of which I shall give a description afterwards. This experiment proves, that the affinity of rhodium for mercury is too strong to enable us to form, at the expense of its oxide, a permuriate of mercury (corrosive sublimate).

I next endeavoured to combine rhodium with sulphur. I reduced the metal to powder in a steel mortar, and I then mixed it with an equal weight of sulphur. When I heated this mixture, the sulphur sublimed without combining with the metal; but towards the end of the process, when scarcely any thing remained but the yellow gaseous vapour of sulphur in the retort, the metal took fire, and produced a sulphuret, which was not, however, saturated with sulphur.

In another experiment, I distilled concentrated nitromuriatic acid off the metal in the state of a very fine powder. The rhodium was not sensibly attacked. After I had in vain evaporated considerable quantities of acid from the metal, I distilled it off at last. The acid had acquired a reddish shade, but had dissolved very little of the rhodium. Dr. Wollaston has informed us that rhodium, in order to be dissolved in acids, must be alloyed with certain metals, as copper or bismuth; but that when alloyed with gold or silver it was insoluble. Hence it appears, that to be able to oxydize rhodium by nitromuriatic acid, it must be combined with another substance, whose propensity to form a double salt with rhodium increases its attraction for oxygen.

We know that chromium, notwithstanding its great combustibility, is scarcely attacked by nitromuriatic acid; but that it is readily oxydated when exposed to heat, especially if in contact with an alkali. I resolved therefore to treat rhodium in a similar manner. I mixed rhodium in fine powder with caustic potash and a little saltpetre, and exposed it to heat in a platinum crucible. As soon as the crucible became red hot, a strong effervescence took place. The metallic powder increased in volume, and became a blackish mass. Water removed the excess of alkali, and left a flea-brown powder, similar to the peroxide of lead. Neither the alkaline lye, nor the water employed to wash the powder, contained rhodium: so that the oxide formed was quite insoluble in water, both cold and hot. Muriatic acid did not dissolve this oxide; but much oxymuriatic acid gas was disengaged when the two substances were heated together.

To examine this oxide with more accuracy, I dried it in a platinum crucible over the flame of a spirit lamp. 148 parts of the oxide thus dried, being treated with muriatic acid, disengaged oxymuriatic gas. After some hours' digestion, I separated the liquid from the undissolved portion. The liquid was evaporated to dryness, and the residue exposed to a slightly red heat. I then dissolved it in water. It left as a residue a little muriate of rhodium, formerly held in solution by the excess of acid. This I added to the undissolved portion. The aqueous solution had a slightly red colour, and yielded by evaporation a quantity of muriate of potash weighing 37 parts. It dissolved completely in water, still preserving its red shade of colour, which was owing to a trace of triple muriate of rhodium. The insoluble portion of oxide thus treated weighed 143·3 parts.

127 parts of this mass being exposed to the heat of a spirit of wine lamp, lost 1·4 parts of moisture. The remaining 125·6 parts were put into a platinum crucible, exactly weighed, and exposed to the strongest heat that I could raise by means of a pair of bellows. The crucible being taken occasionally out of the fire, was found to emit the smell of oxymuriatic gas, which did not stop till the crucible had been exposed an hour to the fire. The brown powder had diminished much in volume, and was now a grey metallic mass exactly similar to platinum, obtained in a similar way from the ammonio-muriate. The metallic rhodium obtained from 125·6 of muriate weighed 97 parts.

To verify this experiment, I decomposed in the same way 100 parts of muriate of rhodium, and obtained 77·3 of reduced metal. These experiments mutually confirm each other, for 126·5 : 97 :: 100 : 77·23. We know from chemical proportions how much oxygen the oxymuriatic gas disengaged from muriate of rhodium contained. Consequently, by finding the composition of the muriate, we find likewise that of the oxide. Muriate of rhodium is composed of

Muriatic acid	17·5544
Oxide of rhodium	82·4456
	100·0000

And the oxide of rhodium is composed of

Rhodium	93·712	100·00
Oxygen	6·288	6·71
	100·000	

We have it now in our power to determine the composition of the oxide, formed by combustion in contact with potash and saltpetre. The 143·3 of muriate of rhodium contain 116·804 of oxide of rhodium, in which there are 7·344 of oxygen (for 127 : 97 :: 143·3 : 109·46). The 37 parts of muriate of potash contain 23·56 of potash, and if we add 23·56 to 116·804, we obtain 140·364. But the oxide employed in the analysis weighed 148 parts. Therefore, 7·636 are wanting. This loss (abstracting the inevitable loss in such an analysis) must be the oxygen carried off by the muriatic acid. But the oxygen lost is equal to the quantity found in the oxide of the muriate. Hence it follows that the oxide, combined with the potash, must contain twice as much oxygen as that in the muriate. The oxygen of the potash combined with the oxide of rhodium is 3·99, and 3·99 × 2 = 7·98. That is to say, that the peroxide of rhodium contains 4 times as much oxygen as the potash with which it is combined. The surplus of oxygen, which we find in this result, appears to draw its origin from the triple muriate with which the muriate of potash was impregnated. The peroxide of rhodium may be separated from the potash by acids; but it cannot combine with them without losing a portion of its oxygen.

Dr. Wollaston has made us acquainted with rhodium in a state of combination to which the metal cannot be brought either by nitromuriatic acid or oxydation by means of heat, and which would certainly have remained long unknown, if rhodium had not been discovered in combination with platinum. This form of combination is the soda-muriate of rhodium, from the colour of which Dr. Wollaston derived the name of the metal. I precipitated a solution of this muriate, prepared by Dr. Wollaston, by adding a small excess of caustic potash. An orange coloured precipitate fell, which after some time divided into two layers. The undermost was thin, very heavy, and yellowish; the uppermost was more bulky, light, and of a reddish colour, like the hydrate of iron. I collected as much of it as I could, without mixing it with the lower layer. I threw it on a filter, and after having well washed it, I allowed it to dry. It resembled exactly dry perhydrate of iron. I reduced it to powder, and exposed it for 24 hours to a heat of 100° Fahrenheit. I found it to be a hydrate of rhodium, containing neither potash nor muriatic acid. I introduced 100 parts of this hydrate into a small retort exactly weighed, and heated it over a spirit lamp. My object was to drive off the water by a moderate heat, and after having ascertained the diminution of weight occasioned by its separation, to expel likewise the excess of oxygen in the oxide. I obtained at first pure water; but the oxide having been too much heated at the bottom of the crucible, appeared to catch fire, and disengaged in a moment its excess of oxygen, and left for residue a brittle greyish mass with the metallic lustre, which weighed 74 parts. Thus the oxygen and water together weighed 26 parts. The grey mass appeared at first to be metallic rhodium, but when I mixed it with some drops of fat oil, and heated it a little, a violent detonation took place, and the rhodium was reduced to the metallic state.

It is evident from the properties of the oxide examined, (as for example the colour of its salts, and its solubility in muriatic acid without disengaging oxymuriatic gas,) that it differs in its state of oxydation from the two oxides above examined; and it appears quite clear, that it must contain more oxygen than they. But if the two oxides above described be $R + O$, $R + 2\,O$, this oxide must be at least $R + 3\,O$. I have proved that gold forms two salifiable oxides, of which the one contains 3 times as much oxygen as the other. I have likewise made it probable, that the purple of Cassius contains an oxide not salifiable, intermediate between the two others; that is to say, that the oxides $Au + O$, $Au + 3\,O$, combine with acids, while the oxide $Au + 2\,O$ does not combine with these bodies, though it has an affinity with other oxides. Now the same thing appears to hold with rhodium. The oxides $Rh + O$, $Rh + 3\,O$, form salts with acids, while the oxide $Rh + 2\,O$ combines only with alkaline bodies. If we calculate, according to this supposition, the result of our analysis of the hydrate of rhodium, we find that 74 of protoxide of rhodium ought to be equivalent to 83·234 of peroxide of rhodium, which of course contains 14 of oxygen. There remain of course 16·766 for the water, which contains 14·8 of oxygen. Hence it would appear, unless the preceding supposition be inaccurate, that in the hydrate the oxide and water contain equal quantities of oxygen. If we consider the protoxide as $Rh + O$, the volume of rhodium will weigh 1490·31.*

(*To be continued.*)

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* As rhodium is a substance very little known, and as few chemists will probably have an opportunity of examining it, I shall here state some observations which I had occasion to make during the experiments given in the text.

I shall begin with the description of the oxides of rhodium. 1. *Protoxide.* To obtain this oxide, reduce the metal to the state of powder, and expose it in an open vessel to a moderate red heat. The metal becomes gradually black, and the protoxide is slowly formed. Its colour is black. When rubbed with a polished hematite, it does not exhibit any appearance of metallic brilliancy. When slightly heated with tallow or sugar, it is reduced with detonation; but, unless it be now removed from the fire, it is oxidized again. The protoxide of rhodium thus obtained is insoluble in acids. It must be employed in its nascent state to form a combination with them.

2. *Deutoxide of Rhodium.*—I call this oxide *oxidum rhodeum*, just as in another memoir I have called the intermediate oxide of tin *oxidum stanneum*. We obtain it by calcining rhodium in powder with caustic potash and a little saltpetre. The potash is removed by water; and if any part of the metal remains, it is separated from the oxide by levigation. The oxide thus gained is light, flea-coloured, and contains between 15 and 16 per cent. of potash. If in place of caustic potash we use the subcarbonate, the oxide combines with a portion of the subcarbonate which water is not able to separate. The same thing happens when the compound of the oxide and potash is long exposed while moist to the air. Diluted sulphuric and nitric acids unite with the potash, but they leave the oxide without forming any combination with it. Muriatic acid forms muriate of rhodium and oxymuriatic acid. This oxide has a strong affinity for alkaline substances. If the caustic potash employed contain lime, the oxide combines with the lime; and if the experiment be made in an earthen vessel, it combines likewise with alumina.

3. *Peroxide of Rhodium* (oxidum rhodicum).—We obtain it by precipitating the sodamuriate of rhodium with caustic potash. The reddish precipitate is the hydrate of rhodium. When heated it gives out its water, and becomes darker coloured; and at a heat below redness takes fire, gives out part of its oxygen, and is reduced to the state of protoxide. We have here the same effect as takes place when *euchlorine* disengages its excess of oxygen and becomes oxymuriatic gas; and the only probable explanation of the curious phenomenon that muriatic acid, instead of dividing the deutoxide into protoxide and peroxide, gives out oxymuriatic gas, and forms muriate of rhodium—the most probable explanation of this phenomenon is, that in the deutoxide the oxygen exists in a state more neutralized, or less electronegative, than in the protoxide; and on this account, when part of the oxygen is disengaged from the deutoxide, the other part combines more intimately with the metal, and produces the phenomenon of fire. On the other side, to form peroxide from the deutoxide, the oxygen of this last ought to be brought to a higher electronegative state, which cannot be done. If we precipitate sodamuriate of rhodium by an excess of caustic ammonia, the precipitate contains ammonia; but it does not explode when heated, and is only decomposed with decrepitation, leaving metallic rhodium. I acknowledge that these curious properties of this oxide made me at first believe that the metallic rhodium which Dr. Wollaston had the goodness to give me was not the same substance which is found in the sodamuriate; but having reduced a portion of rhodium from the sodamuriate, I was soon convinced that my suspicions were unfounded.

Muriate of Rhodium is obtained by treating the deutoxide of rhodium with muriatic acid. It forms an amber-coloured powder, similar to muriate of platinum, which I have described in a preceding memoir. It is insoluble in water, but somewhat soluble in concentrated muriatic acid, communicating to it a reddish colour. The alkalies precipitate it with a colour at first grey, but which afterwards becomes brownish, as the precipitate becomes denser. No acid, not even the nitromuriatic, decomposes this salt. I treated it with nitromuriatic acid containing muriate of soda in solution, in order to convert it into sodamuriate; but it was not altered. Caustic potash does not decompose this muriate, though they be digested together. It is capable of enduring a red heat without decomposition, and can only be reduced to the metallic state by an intense heat long continued.

Sulphate of Rhodium is obtained when the persulphate, to be mentioned below, is exposed to a cherry red heat. It swells up, and disengages sulphuric acid and oxygen gas, leaving for residue a black powder insoluble in water and in acids. Caustic potash separates from it a portion of sulphuric acid. This salt is obtained when sulphuret of rhodium is exposed to a moderate heat.

Persulphate of Rhodium.—This salt is obtained in a way analogous to that by which Mr. Edmund Davy prepared sulphate of platinum. A solution of sodamuriate of rhodium is mixed with hydrosulphuret of ammonia. No precipitate appears at first; but when heat is applied sulphuret of rhodium precipitates. It has the same property with the sulphuret of platinum to acidify in the air while drying; but the property is not so striking in this sulphuret as in that of platinum. The dried sulphuret, being treated with fuming nitric acid, is converted into persulphate of rhodium, part of which dissolves in the acid, and another part remains undissolved in the form of a black powder. As the acid is evaporated, it deposites more of this black powder; and when the whole acid is driven off, the persulphate remains. This salt attracts moisture from the air, and becomes red. It dissolves readily in water; but when the water is evaporated, it does not form a black powder, but a syrupy matter of an orange colour, which swells up in a greater heat, and becomes spongy, like calcined alum. In that state it dissolves slowly in water, just as happens to alum; and though it appears at first scarcely soluble, yet after two or three days we find it entirely dissolved. Caustic potash precipitates from it a pale yellow mass, which appears to be a triple subsulphate.

Permuriate of Rhodium is already known by the experiments of Dr. Wollaston. I shall only add that it is decomposed with more difficulty than the permuriate of platinum; for it is not altered at a temperature which destroys the platinum salt. When the temperature is raised higher, it gives out muriatic acid and oxygen, and leaves for residue muriate of rhodium. I mixed solutions of permuriate of rhodium and common salt, but could not obtain the sodamuriate of Wollaston. The liquid retained its orange colour, and did not become red, even when evaporated to dryness. The common salt appeared to crystallize without entering into combination with the permuriate of rhodium. When I heated the dryness to redness it was decomposed, water dissolved the common salt, and left muriate of rhodium. But when I kept the crystallized sodamuriate in a strong heat for a quarter of an hour, it melted without undergoing decomposition. Its surface was covered with a silvery metallic pellicle, but within it was unaltered, and dissolved in water with its fine red colour, leaving no other residue but the metallic pellicle. Hence it would appear that something else is necessary to form the sodamuriate of rhodium than a mere mixture of peroxide of rhodium and common salt.

ARTICLE III.

Translation of a Letter from M. Malus to the Foreign Secretary of the Royal Society. Communicated at the request of the President.*

SIR, Paris, June 1, 1811.

I HAVE received your letter with the medal which the Royal Society has done me the honour of voting for my Researches on Light, inserted in the Memoirs of the Society of Arcueil. I am very sensible of this distinction, and request you to make the Society acquainted with my gratitude. I am fully aware of the pains you have taken to make my experiments known; and since you take an interest in them, I shall embrace this opportunity of making you acquainted with the further researches which I have made on the subject.

I had announced that when a ray of light was polarized, it might traverse any number of diaphanous bodies without a single molecule being reflected; and I had added that the light, which would have been reflected in the case of a polarized ray, had been transmitted, and not absorbed or destroyed. The following is the direct experiment which you demand of me, and upon which I found this proposition. I place in the direction of a polarized ray a pile of parallel glass plates, and forming with its direction an angle of 35° 25′, and I dispose them in such a manner relative to the poles of the rays of light that no light is reflected. I then make the incident ray turn upon itself without changing its place, and preserving the same inclination with respect to the pile. When it has revolved the fourth part of the circumference, it is totally reflected by the successive action of the plates, and it ceases to be perceived at the extremity of the pile. After half a revolution it begins to be transmitted again. Thus when we perceive the greatest quantity of refracted light, there is no reflected light; and when we perceive the greatest quantity of reflected light, there is no light refracted beyond the pile. It is therefore very evident that the light reflected in one case is transmitted in the other, and *vice versa*.

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* The publication of this letter was deemed necessary in consequence of the observations contained in the introduction to Dr. Brewster's paper inserted in the preceding Number of the *Annals of Philosophy*.

It is not necessary to observe, that to turn a polarized ray on itself I employ a ray formed by the ordinary refraction of a piece of Iceland crystal, the faces of which are parallel to each other, and perpendicular to the direction of the ray. By turning round this substance in its own plane, I change the direction of the poles of the rays without varying their direction or intensity.

I shall now state an observation which throws a new light upon this subject. When an ordinary ray falls upon a plate of glass at an angle of 35° 25′, all the light reflected is polarized; but the transmitted ray contains (proportional to the rays reflected) a certain quantity of light polarized in a direction diametrically opposite. If we expose this ray to the successive action of different plates, the quantity of transmitted light polarized accumulates incessantly: so that, after a certain number of transmissions, all the light reflected is polarized in one direction, and all the light refracted is polarized in another. If we receive each of these rays upon rhomboids of Iceland spar, the principal section of which is parallel to the plane of incidence, the light reflected by the glasses is refracted in the ordinary way, and the light transmitted, in the extraordinary way. This has led me to the following conclusion, that as often as a quantity of light is polarized in one direction an equal quantity is polarised in the other.

Metallic surfaces appeared to me to present the phenomena of polarization in a very incomplete manner. In fact, the ray reflected from them at all incidences is always susceptible of being divided into two pencils by the reflection of Iceland crystal. We observe, it is true, when the incidence is very great, that one of the images becomes faint while the other increases in intensity; but the phenomenon is never sufficiently apparent to enable us to determine at what angle it is a maximum. I have overcome that difficulty by the following experiment.

I receive upon a metallic mirror a ray already polarized, but so that the plane of reflection, which passes through the incident and reflected ray, makes an angle of 45° with the rectangular poles of the ray. If the incidence is very small, or very great, the ray is not depolarized. When subjected to the action of a rhomb of Iceland spar, it is always capable of being refracted in a single ray; but under a mean incidence, and one peculiar to each metal, it appears completely depolarized, so that it may be always divided into two rays by Iceland spar. Hence metallic surfaces, as well as other bodies, have a determinate angle at which they change the poles of the ray which they reflect. But since in this case the plane of reflection makes an equal angle with the two poles, one half of the light is polarized in one direction, and the other half in the other; so that the reflected ray assumes the properties of a direct ray.* This experiment shows us why, if we employ with metallic mirrors the same method as with diaphanous bodies, the determination of the proposed angle becomes impossible. In fact, when natural light falls under the angle proposed, the reflected ray contains at once the molecules polarized in one direction and in the other; so that, when decomposed by Iceland spar, it presents the same phenomena as the natural ray which is reflected without polarization under the greatest and smallest incidences, which in that case renders the limit indeterminable. When we subject to the reflection of the mirror a ray already polarized, we avoid that inconvenience; because, instead of observing as with transparent bodies the angle under which the polarization is most complete, we observe, on the contrary, that in which the depolarization is in appearance the most complete.

Thus, for metallic substances, we must employ the reflection of a ray already polarized, and taking care that the poles of the ray form an angle of 45° with the plane of incidence, and must observe at what angle light appears depolarized like a natural ray: for diaphanous substances, on the contrary, we must employ the reflection of a natural ray, and observe the angle at which the light appears completely polarized. The angle in both cases will be determined with the same accuracy.

It is now therefore proved that all bodies in nature, both opaque and diaphanous, polarize light entirely when they reflect it under a certain angle; and that the metals, which alone appeared to constitute an exception to this law, on the contrary polarize a greater quantity of light than other bodies, since they reflect a greater quantity.

From the preceding experiments there results a remarkable fact. Place before a luminous body a set of parallel glass plates, and increase their number till the body ceases to be seen when viewed perpendicularly through them: if you look at it obliquely through the same plates you will begin to see it again. Under the angle of incidence 54° 35′ its light will have the greatest intensity. Beyond that limit it begins again to disappear. This phenomenon proceeds from this circumstance, that the light transmitted obliquely loses by polarization the faculty of being reflected.

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* What distinguishes metals from transparent bodies is, that these last refract all the light polarized in one direction, and reflect all polarized in the other; while metallic bodies reflect what they polarize in both directions. It being understood, however, that they possess in part the power of all other opaque bodies of absorbing a greater proportion of that kind of ray which diaphanous bodies transmit.

I do not consider the knowledge of these phenomena as more favourable to the system of emission than to that of undulations. It demonstrates equally the insufficiency of the two hypotheses. In fact, how can we explain either by the one or the other why a polarized ray can pass totally at a certain inclination through a diaphanous body without experiencing that partial reflection which takes place at the surface of bodies in ordinary cases?

It remains for me to thank you for the eriometer which you were so good as to send along with your letter, by means of which we may judge of the fineness of different wools and other very small bodies. It is a very useful application of the laws which you have deduced from your ingenious hypothesis respecting the combined movements of light, and a new proof of the advantages which the arts derive from the progress of the sciences.

I request you to present to the Royal Society one of the copies of the Theory of Double Refraction which accompany this letter, and to accept the other as a mark of my particular esteem.

I have the honour to be, Sir,
With the greatest respect,
Your most humble and obedient servant,
MALUS,
Member of the Imperial Institute
of France.

ARTICLE IV.

On the Number of Inhabitants in Russia, and on the Progress of its Population, according to the Statements made by order of Government. By C. T. Hermann.

(*Concluded from* p. 173.)

PART II.

Of the Progress of Population in Russia.

THE first *revision* of 1722 gave 5,794,928 males, which, supposing an equal number of women, makes a population of 11,589,856 individuals. How much ought we to add for the new acquisitions in which the revision did not take place?

An enumeration made in Little Russia* in 1768 gave 955,228 inhabitants; another, made in Finland in 1755, gave 117,998. Esthland, in 1773, had 176,000; Livonia, 447,360. All these make a sum total of 1,696,586 persons. But these enumerations being made 20, 30, 50 years after the first revision, it is possible that the population may have increased or diminished during the interval. If we compare these data with the enumeration made in 1805, we shall find that Finland, in 49 years, has gained 64,392 inhabitants; Esthland, in 31 years, 36,948; and Livonia, 138,097: making a sum total of 239,437. The population in the provinces surrounding the Baltic, then, has gained about ¼th during the latter half of the eighteenth century. If we compare the population of Little Russia above stated with that of the governments of Tschernigow and Pultawa, we find in 1804 a surplus of 1,465,465 individuals above the enumeration of 1768. According to this statement the population has more than doubled during the last 50 years. This result corresponds very well with the observations made on the registers of births and deaths, that the progress of population is very slow in the Baltic provinces, and very rapid in Little Russia. It has gained of late especially by the commerce of Odessa, the price of land has risen considerably, and even the fertile steppes have been cultivated.

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* Hermann, Journal Statistique, t. i. partie ii, p. 14.

If we admit the same proportion in the progress of population in these provinces during the first half of the eighteenth century, which is certainly admitting a great deal, we must deduce from the above stated population of the Baltic provinces one-fourth, and there will remain 555,919; and one-half of the population of Little Russia, in 1768, leaving 477,614. According to this statement the population of all the provinces acquired after 1722 may be estimated at 1,033,533.

It remains for us to determine what may have been the number of free persons not included in the revision. As at the last revision, of 1796, there were 16 millions of males included in the list of those that paid the direct impost for one million that did not pay, we may suppose that at the first revision, in which the number of revisionaries was five millions, there were 300,000 male freemen, making with their wives the number of 600,000.

According to these calculations, the probable population of Russia in 1722 will be

Revisionaries	11,589,859
Free individuals	600,000
Conquered provinces	1,038,533
	13,223,392

The author of the essay on the commerce of Russia (Le Clerc), a work published in 1777, states the population at 14 millions: Hermann, at the same. This number is probable; but when Voltaire reckons the population during the last years of Peter the Great at 18 millions, he confounds a latter period with the time of that monarch. It appears to me that 14 millions is the most probable number, if we consider the imperfection inseparable from a first enumeration, and the uncertainty of the calculations respecting the newly conquered provinces.

The second revision, in 1742, gives 6,673,167 males; and supposing an equal number of females, we have 13,346,334 for the inhabitants of Russia at that time. To this we must add the conquered provinces and the free individuals. As we subtracted a quarter from the population of the Baltic provinces in 1722, the abstraction of one-eighth will be sufficient for their population in 1742. The remainder is 648,689; and subtracting a quarter from the population of Little Russia in 1768, there remains 706,421; making a total of 1,355,110 for the population of the conquered provinces. As the number of revisionaries had increased by a million since 1722, we must increase the number of free men at least by 50,000, considering the progress of industry and the amelioration of the administration. Hence the population in 1742 will be

Revisionaries	13,346,334
Free individuals	700,000
Conquered provinces	1,355,110
	15,401,444

Hermann admits for 1742 the round number of 16 millions. This is a very probable estimate, as the enumerations in Russia are always under the truth.

The third revision, in 1762, gives 7,363,548 males, which supposes a total of 14,727,096 individuals. As the time of the revision is nearly a mean of the enumerations of the conquered provinces above stated, we may take their population at 1,696,586. The revisionaries being nearly one-half of what they are at present, we may suppose the same to hold with the free men, which would make their number 400,000. According to this, the probable population of 1762 is as follows:—

Revisionaries	14,727,096
Free individuals	800,000
Conquered provinces	1,696,586
	17,223,682

Marshal, in 1768—1770, and Williams, in 1768, admit 18 millions; Leveque, in 1782, and Le Clerc, in 1783, 19 millions; Schlozer and Busching, in 1765, 20 millions; and Hermann is of this last opinion. I conceive the true number in 1762 to lie between 18 and 19 millions.

The fourth general revision, in 1782, gives 12,838,529, and with the females, 25,677,058; or, according to Hermann, 26,358,822. The two capitals, the military, and the Nomades, are not included in this number. These at present amount to 2,960,000. At that time we may suppose them to have amounted to two millions. According to this statement, the population of Russia in 1782 would have been between 27 and 28 millions. Crome, in 1785, admits 23 millions; Susmilch, 24; Pleschtscheef (not reckoning the clergy, the civil government, the military, and the Nomades,) admits 26,617,698 in 43 governments; while Hupel, in 1780—1790, and Hermann, admit 28 millions.

The fifth revision, in 1796, gave 17,816,370 males, which, supposing an equal number of females, makes the population amount to 35,632,740; or, according to the datum 16,223,229, (which I consider greatly below the truth,) 34,038,599. If we add the capitals, the military, and the Nomades, reckoning them at 2,960,000, the population in 1796 will amount to 36,998,599; Busching and Beausobre make it 30 millions; Schlozer, 33; Hermann, 33½; Meusel, between 35 and 36; and Storch, 36.

According to these data the progress of population in Russia, produced partly by the improvement of the interior, partly by new acquisitions, has been as follows:—

In 1722	14 millions	
1742	16	after 20 years
1762	19	after 20
1782	28	after 20
1796	36	after 14
1806	41	after 10

This astonishing progression has proceeded, in a great measure, from new acquisitions. It would be interesting to be able to determine nearly the progress of the Russian population, independent of the new acquisitions.

We admit for Little Russia and the Baltic provinces the number made known by the enumerations of 1755, 1768, and 1778, which gives a total of 1,696,586; and we add the new acquisitions since 1773, according to the data published by General Opperman on his map of 1796, made by order of Government, in order to point out the new limits. According to this author, Russia acquired,

By the first division of Poland in 1773, 1,226,966 individuals:

By the Peace with the Ottoman Porte in 1774 and 1783, 171,610:

By the Peace with the same Power in 1791, 42,708:

By the second partition of Poland in 1793, 3,745,663:

By the re-union of Courland, 387,922:

And by the last partition of Poland in 1795, 1,407,402:

The total of the acquisitions since 1773, 6,982,271:

Adding the Baltic provinces and Little Russia, we get 8,678,857.

This is the total amount of the population of the countries conquered down to 1795.

But all this was obtained by means of first enumerations, which were necessarily incorrect. Those made down to 1804 ought to be more accurate. The administration ought to have acquired great influence, especially after the organization of the governments in

1775. It will be interesting, therefore, to know the effect of these causes as exhibited by the last enumeration, in 1804, which I possess.

Little Russia includes the governments of Kiew, Tschernigow, Pultawa, Slobod-Oukrainskoi, and a part of Catherinoslaw and Koursk, to which we must add the country of the Cossacks of the Don as peopled by the inhabitants of Little Russia. All this vast country was the boundary between the Turks and Tartars, called the Oukraine. Its population in 1804 was as follows :—

Governments.	Males.	Females.
Kiew	574,217	538,404
Tschernigow	534,712	538,570
Pultawa	713,772	732,639
Slobod-Oukrainskoi ...	420,304	418,781
Catherinoslaw	210,815	183,363
Cossacks of the Don ..	161,100	194,521
	2,614,920	2,606,278
	5,221,198	

The Swedish provinces are, a part of Carelia and Ingria, constituting at present the government of St. Petersburgh ; Finland ; Esthland ; and Livonia. Their population in 1804 was as follows :—

Governments.	Males.	Females.
St. Petersburgh	268,748	270,920
Finland	94,397	87,993
Esthland	107,357	105,591
Livonia	290,014	295,443
	760,516	759,947
	1,520,463	

So that the number of inhabitants in the Swedish provinces and in Little Russia is 6,741,661.

If we compare this number with the preceding enumerations, we see that the Swedish provinces have gained one-fourth, and that Little Russia has nearly doubled ; for it is certain that in this last enumeration all the provinces belonging to Little Russia in its greatest extent have not been included.

The Polish provinces acquired from 1773 to 1795, including Courland, are, White Russia, Lithuania, and the Polish Oukraine, or the governments of Minsk, Vitebsk and Mohilew, Grodno and Vilna, Podolia and Volhynia. The state of their population in 1804 was as follows :

Governments.	Males.	Females.
Minsk	431,586	426,940
Vitebsk	343,716	330,624
Mohilew	403,614	397,381
Grodno	300,278	290,782
Vilna	465,224	460,046
Podolia	579,215	556,870
Volhynia	564,586	522,182
Courland	191,910	189,366
	3,280,129	3,174,191

6,454,320

According to General Opperman, the population of these provinces in 1796 amounted to 6,767,953. Hence it appears that the population of Poland is stationary.

The Turkish provinces are, Cherson, the Tauride, the country of the Cossacks of the Black Sea, and the remainder of Catherinoslaw, to which may be added Caucasia. The population of these provinces is as follows:—

Governments.	Males.	Females.
Cherson	145,814	124,321
The Tauride	102,826	88,864
Cossacks of the Black Sea	20,240	9,155
Caucasia	34,849	29,240
	303,729	251,580

555,309

As the Cossacks of the Black Sea have very few women, and still retain many of the customs of their ancestors, the famous Saparoques, the preceding statement is probably correct. According to General Opperman, there were in the Turkish provinces conquered in 1774, 1783, and 1791, 214,318 individuals of both sexes. This small population, in a tract of country so immense, has increased undoubtedly in consequence of a more regular administration, but not so much as would appear at first sight; for we must strike off the Cossacks of the Black Sea, Caucasia, and the Russian and foreign colonies domiciliated in these countries. Besides, if we consider the imperfection of a first enumeration, it is but reasonable to suppose that General Opperman's estimate is too small.

Thus it appears that the population of the countries acquired since 1773 was in 1804 as follows:—

Little Russia	5,221,198
Swedish provinces	1,520,463
Polish provinces	6,454,320
Turkish provinces	555,309
	13,751,290

According to the preceding data, we must subtract from the total of the population of Russia for the provinces acquired

1,033,533	from the 14 millions in	1722	Remains 12,966,467
1,355,110	16	1742	14,644,890
1,696,586	19	1762	17,303,414
8,678,857	28	1782	19,321,143
13,751,290	36	1796	22,248,710
13,751,290	41	1806	27,751,290

The last column gives us the rate at which the population of Russia proper has increased.

From this it follows that the population of Russia, excluding the conquests since the time of Peter the Great, gained in 20 years, between 1722 and 1742, 1,678,423, or 83,921 annually:

In the 20 years between 1742 and 1762, 2,658,524, or 132,926 annually; that is to say, 49,005 more annually than during the first period:

In the 20 years between 1762 and 1782, 1,676,253, or 83,812 annually; less by 49,114 than during the second period:

In the 14 years between 1782 and 1796, 2,927,567, or 146,378 annually; more by 62,566 than during the preceding period:

In the 10 years between 1796 and 1806, 5,000,000, or 200,000 annually; more by 53,622 than during the preceding period.

We see by the preceding table that the population of Old Russia has more than doubled, or that it is at present to what it was in 1722 as 2⅙ to 1. We see also that the progress of population has not been uniform, that it has had accelerations and retardations, that the most favourable periods was during the reign of the Empress Elizabeth, between 1741 and 1761, and the years of the peace of Catharine II. between 1782 and 1796. The population still advances in the latter periods, but the rate is slower. What may be the cause of these phenomena?

The population of Russia has more than doubled during the last century, while Smith supposes that the population in civilized countries only doubles once in 500 years. It has doubled in consequence of a better regulated administration, of the security which the government has procured to the nation, of the capitals of foreigners placed in the country, and which for a long time constituted the soul of the commerce of the interior; in consequence of the progress of national industry, which was the result; of the increase of knowledge, by new commercial connections with the other countries of Europe, and by the means of instruction furnished by the Government to the inhabitants of Russia; and, finally, in consequence of the removal of several obstacles which opposed the progress of industry, as the abolition of the *douanes* of the interior under the reigns of the Empresses Elizabeth and Catharine II., the improvement of the roads, and the multiplication of canals.

What a dismal picture does Russia present to us in the 15th, 16th, and 17th centuries. Josafa Barbaro, in 1436, reports that from Moscow to the frontiers of Poland the whole country was a desert, the villages, burnt and abandoned, offered no other accommodation to strangers but a place to kindle a fire. Contarini confirms this statement in 1483. Meyerberg, in 1661, found between Waesma and Mosaisk, a distance of 130 wersts,* only a single village. The road between Smolensk and Moscow was dangerous, according to Lyseck, in 1675, on account of the wolves that attacked travellers. Ulfeld, the Danish Ambassador, in 1625, found the country between Moscow, and Novgorod, and Plescow, quite laid waste by the civil wars under Iwan Wasilewitch II. Possevin, in 1581 and 1582, travelled whole days in the interior of Russia without meeting with a single individual. The whole country between Kasan and Astracan was a desert. Even the cities had suffered a good deal. Possevin estimates the population of Moscow at 30,000; that of Novgorod was diminished by the plague to 3000: and Kiew, in the time of Herberstein (1516) was almost in ruins. Besides the devastations committed by the civil wars and by foreigners, the number of imposts, and the severity of the commissioners who levied them, depopulated the northern provinces which had not suffered from these disasters. In the year 1588 there were, according to Fletcher, 50 villages abandoned between Wologda and Jaroslaw. Bread was almost unknown at Ustiug, and on the Dwina, in the time of Herberstein. Famine and pestilence often ravaged the melancholy remnants of this unfortunate population, as in 1525, in 1601, and in 1615. The city of Novgorod lost in one winter 18,000 individuals, constituting almost the whole of its population.†

It is not to the mildness of the climate, nor to the fertility of the soil, that we are to ascribe the rapid increase of population during the eighteenth century; but to a better organized administration, and the security which resulted from it. An infant state, supposing it tolerably well governed, and connected with states long civilized, ought to make prodigious progress in improvement and population.

That period of the glorious reign of the Empress Catharine II. in which she occupied herself with the amelioration of the government of the interior was particularly propitious to the progress of population. The organization of the governments in 1775 was the great political institution which procured the inhabitants a greater degree of security and happiness. The manifesto in 1782 respecting the liberty of working the mines, the establishment of normal schools in 1783, the rights granted to the nobility in 1785, the improvement of the great roads in 1786, and especially the establishment of the Bank in the same year, were all calculated to promote the happiness of the subjects, as much as that depended upon Government. The bank from its commencement had a very happy effect upon the progress of agriculture. This great Empress removed several difficulties which stood in the way of the happiness of her subjects, as the want of liberty to be industrious, the want of communications, the want of knowledge, and of a medium of circulation.

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* A werst is about two-thirds of an English mile.—T.

† Meiner's Comparaison de la Russie Ancienne et Nouvelle, 1798, b. i. ch. i.

The population of Russia has more than doubled during the eighteenth century. Have we reason to expect the same progress during the 19th?

If we consider only the extent of the surface capable of cultivation, which is supposed to amount to 80 millions of square miles, we shall conclude that Russia is capable of supporting 960 millions of inhabitants, or almost as many as at present exists on the earth.

If we consider the surplus of births as a total gain to the population, this surplus, amounting at least to half a million annually, would in 32 years amount to 60 millions, in 56 to 80 millions, &c.

But experience shows us that the progress of population does not depend solely upon the extent of soil capable of cultivation. There are spots left uncultivated in the countries in which agriculture has made the greatest progress, even in England, Flanders, and Lombardy. This progress depends still less on the surplus of births. Every where the number of births exceeds that of deaths. The population is always proportional to the state of national wealth. Hence the most decisive proof of the prosperity of a country is the increase of the number of its inhabitants. In Russia the population has more than doubled in 84 years. Hence we may conclude that its agriculture, the principal branch of its industry, has also doubled. In France, during the reign of Henry IV. in 1593, there were 16 millions of inhabitants; and during the time of Louis XV. in 1723, there were 25 millions. Its population has not even doubled in 130 years, because the state of its agriculture was languishing (see Arthur Young and Du Pradt), and its commerce cramped by England. Great Britain, on the contrary, had in 1758 8 millions of inhabitants,* in 1794 the population was reckoned

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* It is needless to point out to the English reader that this statement applies not to England but to Great Britain and Ireland. If the author's reasoning be correct, Ireland is the country that has been the most flourishing in Europe during the course of the last century, for its population has certainly increased at the greatest rate.—T.

12½ millions,[*] and since 1804 it has been reckoned 15 or 16 millions.[†] Thus it has gained 4 millions in 36 years, and 3 or 4 millions during the last 10 years. If these data are correct, there must, during the first period, have been an annual surplus of 111,000 inhabitants, and during the last a surplus of 300,000; and the population would have almost doubled in half a century.[‡] This progress is astonishing; but the progress of the industry of England is equally so, especially since the time of William III. under whom the rights of the people were better understood and irrecoverably fixed. However, the progress of population in England is not without a parallel; for Smith acknowledges that in the British North American colonies, the United States, the population doubles in 20 or 25 years. Here labour is so productive that a numerous family, instead of being an expense, is a source of opulence to the parents. The labour of each child before he quits his father's house is estimated as 100*l.* sterling. Here the demand for workmen, and the funds for supporting them, increases more rapidly than their number. Thus there are different scales for the progress of population, all independent of the extent of land capable of cultivation, and of the excess of births above deaths; but all directly proportional to the increase of national wealth, which in some countries has been more slow, in others more rapid.

According to these principles, founded on experience, what ought we to predict respecting the future progress of Russia? Her agriculture has increased prodigiously during the eighteenth century, so much so that in the well cultivated governments round Moscow it can extend no further without the ruin of the forests and meadows which remain. It may still gain a great deal by improvements of the soil; but the requisite capital can only accumulate slowly by means of commerce, as is shown by the examples of England, Flanders, and Lombardy. Hence we cannot expect a rapid improvement in the agriculture of Russia during the nineteenth century. Here manufactures, still inferior to those of England, Germany, and France, are, notwithstanding, sufficient for the markets which they find at present in the interior and in Asia. Here the productions must be regulated by the demand. As to the manufactures for the foreign market, they depend upon foreign commerce; and this commerce has been carried on abroad by foreigners, and at home by means of capital furnished by foreigners. The increase of Russian commerce depends upon peace, and upon the augmentation of knowledge. On these accounts we cannot expect so rapid an increase of national riches, and consequently of population, during the nineteenth century as took place during the eighteenth.

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[*] Stuart's Researches on the Principles of Political Economy, vol. i. ch. xv. p. 172.

[†] Meusel, Statistique, second edition, of 1794, n. 216.

[‡] The reader will be aware that the data are incorrect. The real rate of increase during the last century is exhibited in the table published by the House of Commons, and inserted in the first Number of the *Annals of Philosophy.*—T.

Experience has proved the accuracy of this reasoning. The progress of the population has become slower since the fifth revision. The annual surplus of 60,000 has been reduced to 50,000 during the last 10 years.

To establish this fact I have compared the statements respecting the population of 25 governments of Old Russia, in which the surplus of births is the most considerable according to the fourth revision, of 1782, with the statements of their population according to the fifth revision, of 1796, and with the general enumeration of 1804. These governments are, Moskwa, Toula, Kalouga, Jaroslaw, Orel, Koursk, Wladimir, Resan, Pensa, Kasan, Twer, Smolensk, Tambow, Nigegorod, Plescow, Woronesch, Simbirsk, Kostroma, Waetka, Novgorod, Saratow, Perm, Orenbourg, Wologda, Olonetz. The results are as follows :—

The statement respecting the populations of the 25 governments, according to the fourth revision, of 1782, gives 9,360,799 males :

That of the fifth revision, of 1796, 14 years after, 10,228,672:

That of the enumeration of 1804, 8 years after, 9,989,531 :

So that the population gained during the first period 867,873, and lost during the second period 239,141.

In the first period there are only three governments whose population has diminished ; Kalouga, Kostroma, and Woronesch. All the others had increased. But during the second period Moscow alone gained considerably, namely, 100,000 males ; Woronesch, which had lost before, gained 150,000 males ; and, finally, Waetka gained 37,000. Koursk and Orel have gained a few thousands. Toula, Jaroslaw, and Perm, a few hundreds. All the other 17 governments have lost, and some of them considerably, Thus Tambow has lost 88,000 men, Nigegorod 55,000, and Simbirsk 110,000, in 8 years !

It is to be observed that the governments which have been long well cultivated, as Toula, Jaroslaw, Kalouga, Twer, Plescow, Kostroma, Smolensk, Wladimir, have neither gained nor lost much. The population, and of course the industry, is stationary. The governments less improved, as Waetka and Woronesch, have gained a great deal ; and the governments richest in corn, as Tambow, Nigegorod, Simbirsk, have lost the most.

The rapid progress of population between the fourth and fifth revision is the natural effect of the sensible progress which agriculture made in consequence of the many good new institutions, and especially the establishment of the bank. These institutions and new funds have produced their effect. At present the ancient sources of national wealth flow less abundantly, and it is not easy to open new ones. I presume, then, that the population of Russia will remain a long time between 41 and 43 millions. However, unforeseen circumstances may give a considerable population to the south of Russia. For example, the astonishing commerce of grain at Odessa between 1800 and 1805 increased the value of all the lands as far as Kiew, and even the fertile steppes were cultivated. Workmen were wanting, and even half the produce was offered to those who would gather in the other half. The commerce of Taganrok likewise furnishes ground for hope: and agriculture appears to be making some progress among the Nomades.

A country is sufficiently peopled when almost all the inhabitants are in easy circumstances. Such a population alone is desirable, and useful to Government. A country is not sufficiently peopled when the demand for workmen, and the means of maintaining them, are excessive, as in some of the southern provinces of Russia. A country suffers from its population when the thousands of rich are obliged to maintain the millions of poor. Such an abusive population must either perish, or leave the country, or produce revolutions.

In the old provinces of Russia there are not any beggars. The inferior class of nobility, which is the most numerous, live at their ease in the country. They constitute the true Russian farmers: their existence depends on the progress of agriculture, and it prospers in their hands. The Russian peasants are very far from being unhappy. They are in general much more at their ease than the same order of men was in France in the time of Lady Mary Wortley Montagu, who met none else than beggars between Lyons and Paris. Even the number of rich peasants is considerable; and it must become every day greater, as long as they preserve the ancient simplicity of their manners. Their savings necessarily accumulate under the form of capitals, and these capitals become by degrees productive; for many peasants have already abandoned agriculture, and occupy themselves with other branches of industry, as arts, and trades, and even commerce. It is only in Finland and the Polish provinces that the peasants are poor.

The power of a state does not depend upon the number of its inhabitants alone, but upon their wealth and activity. Russia has no reason to complain in this respect. She is sufficiently peopled for the actual state of her national riches. What would not this empire become if her population were more concentrated?

Meteorological Journal of the Weather at Derby and Sidmouth during 1813. Communicated by Dr. Clarke.

Annual Meteorological Table for Sidmouth, Devon. 50° 41′ N. lat. 3° 13′ W. long. By Dr. Clarke.

1813.	THERMOMETER.—North Exposure.																BAROMETER.								WEATHER			WINDS.				RAIN
Months.	Highest by Six's register.	Day.	Wind.	Lowest by Six's Register.	Day.	Wind.	Mean of the extremes.	Mean of the highest.	Mean of the lowest.	Mean of the two.	Mean at 9 morn.	Mean at 2 aftern.	Mean of 9 A.M. and 2 P.M.	Mean at 11 night.	Mean of 9 A.M. 2 P.M. & 11 P.M.	Greatest variation in 24 hours.	Day.	Highest.	Wind.	Day.	Lowest.	Wind.	Mean for the month.	Greatest range in 24 hours.	Fine.	Cloudy.	Wet.	N and N E.	E and S E.	S and S W.	W and N W.	In inches and decimals.
Jan.	49°	6	S W	25°	28	N	37°	41°	34°	38°	36°	41°	39°	34°	37°	13°	31	30·59	N	8	29·40	N	30·14	41	14	12	5	15	5	4	8	—
Feb.	51	28	N W	31	10	W	41	47	39	43	44	47	45	41	44	13	28	30·57	N W	14	29·22	S W	29·93	43	8	2	18	—	6	15	10	3·00
March	58	29	W	28	12	N E	43	51	38	45	48	51	50	41	47	16	7	30·62	N	20	29·74	S	30·26	46	21	1	9	10	3	6	14	·85
April	59	13	S W	31	2	N W	45	51	39	45	50	51	50	40	47	10	10	30·69	E	1	29·33	S W	30·16	58	20	0	10	7	4	9	13	1·75
May	65	29	N	40	26	N W	53	59	47	53	57	59	58	50	56	11	31	30·26	S W	14	29·49	S W	29·86	43	14	5	12	2	8	10	13	2·75
June	72	28	S	42	17	S W	57	62	48	55	61	62	61	49	57	23	26	30·34	N E	9	29·61	S E	30·08	30	18	1	11	7	7	14	2	1·22
July	70	31	N W	42	7	S E	56	66	48	57	62	66	64	48	59	8	4	30·39	N	25	29·63	S W	30·02	40	18	1	12	4	4	8	15	2·96
Aug.	71	2	S W	40	23	W	56	67	48	57	63	61	65	—	—	8	24	30·42	S W	5	29·65	W	30·20	45	23	1	7	6	5	8	12	·85
Sept.	69	17	S W	40	7	S W	55	62	49	56	59	62	61	53	58	13	17	30·40	S W	6	29·38	S W	30·08	35	18	4	8	7	4	11	8	1·70
Oct.	65	5	S W	30	29	N E	48	56	46	51	52	56	54	49	53	20	26	30·23	N E	17	28·84	S W	29·72	79	13	—	18	8	2	16	5	3·80
Nov.	56	13	N W	32	3	N	44	49	41	45	45	49	47	43	45	14	4	30·44	N	17	29·46	N E	29·87	50	11	9	10	9	3	6	12	2·38
Dec.	52	18	S	25	13	N E	39	43	36	40	40	43	42	40	41	15	27	30·52	S E	2	29·14	S E	29·87	52	20	3	8	12	7	4	8	1·35

ANNUAL RESULTS.

Thermometer.		*Wind.*
Highest, June 28	72°	S
Lowest, Jan. 28	25	N
Mean of the extremes	48	
Mean of the highest	54·5	
Mean of the lowest	42·5	
Mean of the two	48·5	
Mean of 9 morning	51·5	
Mean at 2 afternoon	54·5	
Mean of 9 A.M. and 2 P.M.	53·	
Mean at 11 night	44·5	
Mean of 9 A.M. 2 P.M. and 11 P.M.	49·5	
Greatest variation, June 5—6	23	

Barometer.		*Wind.*
Highest, April 10	30·69	E
Lowest, Oct. 17	28·84	S W
Greatest range, Oct. 17—18,	0·79	
Mean	30·01	

Wind	*Times*	*Wind*	*Times*
N N E	87	S S W	111
E S E	58	W N W	120
	145		231
			145
			376

Weather.	*Days.*
Fine	198
Cloudy	39
Wet	128
	365

	Inches.
Rain for 11 months	22·61

Annual Meteorological Table for Derby, lat 52° 58′ N. long. 1° 30′ W. By Mr. Swanwick.

1813.	Register Thermometer.—North Exposure.										Barometer.—{170 feet above the level of the sea.								Weather.		Winds.				Rain.
Months.	Day.	Highest for the day.	Wind.	Day.	Lowest in the night.	Wind.	Mean for the day.	Mean for the night.	Mean for the month.	Greatest variation in 24 hours.	Day.	Highest.	Wind.	Day.	Lowest.	Wind.	Mean for the month.	Greatest range in 24 hours.	Fair.	Wet.	N and N E.	E and S E.	S and S W.	W and N W.	Inches and Decimals.
Jan.	6	50°	S	29	17°	S W	37	31°	34	13°	25	30·46	E	14	29·38	N E	30·00	·41	20	11	5	16	10	6	·30
Feb.	21	54	S	4	31	W	46	36	41	10	3	30·40	N	14	29·10	S	29·73	·50	9	19	1	0	17	10	2·14
March	28	60	W	31	30	S W	49	35	42	13	26	30·47	N W	21	29·62	S	30·14	·41	23	8	5	0	9	17	·72
April	12	70	W	3	29	S W	54	38	46	14	13	30·36	S	1	29·05	W	29·91	·70	22	8	8	3	9	10	·59
May	30	72	S	1	38	N E	58	44	51	19	30	30·10	S	21	29·34	S W	29·70	·45	10	21	6	3	13	9	3·25
June	2	76	N E	23	38	W	65	46	55	12	22	30·32	N	9	29·35	S W	30·09	·35	19	11	14	5	6	5	1·17
July	30	79	N E	9	44	W	67	48	58	14	6	30·30	S	20	29·30	S W	29·78	·79	19	12	3	1	12	15	1·93
Aug.	11	72	S	25	37	N E	63	47	55	11	26	30·28	N	6	29·50	S E	30·02	·30	23	8	8	1	6	16	·56
Sept.	17	66	S	10	36	S	59	45	52	13	17	30·31	S	6	29·11	S	30·00	·50	18	12	9	5	12	4	1·42
Oct.	5	62	W	19	26	E	50	41	46	14	23	30·24	N E	17	28·93	N	29·81	·80	15	16	12	4	18	7	5·24
Nov.	20	52	S	30	26	S E	44	34	39	9	4	30·51	N	17	29·38	N W	29·90	·50	16	14	4	4	10	12	1·62
Dec.	18	48	S W	30	22	W	38	32	35	9	11	30·32	E	18	29·25	S W	29·88	·42	21	10	9	3	11	8	4·83

ANNUAL RESULTS.

Thermometer.		*Wind.*
Highest, July 30	80°	S
Lowest, Jan. 29	17	S W
Greatest variation, May 28—29	19	
Mean for the day	52·6	
Mean for the night	39·8	
Annual mean	46·3	

Barometer.		*Wind.*
Highest, Nov. 4	30·51	N
Lowest, Oct. 17	28·93	N
Greatest range, Oct.	·80	
Annual mean	29·91	

Wind.	*Times.*
N and N E	84
E and S E	39
S and S W	123
W and N W	119
	365

Weather.	*Days.*
Fair	215
Wet	150
	365

	Inches.
Rain	20·34

Article VI.

Meteorological Table of the Weather at Plymouth for January, 1814. By James Fox, jun. Esq.

(To Dr. Thomson.)

Sir, Plymouth, Portland-square, Feb. 1814.

Not having observed any account of meteorological observations in the west of England, either in the *Annals of Philosophy*, or any other scientific work, I conclude none have been kept; or if so, at least have not been communicated for public information: under this impression I take the liberty of saying, that I have for some time past registered the following observations, which are much at your service if you think them worthy notice:—the state of the barometer, ditto thermometer, ditto wind; quantity of rain, snow, and hail; fogs, thunder, or other occurrences.

Having stated thus much, it may not be improper to describe the situation of my residence. It appears to me good for the purpose, being one of a row of houses intended to form the side of a square, which has been recently marked out in a field about a furlong distant from the town of Plymouth.

I have a good sized garden attached to it, which adjoins on either side to others, the one at the end being a *field* planted as a kitchen garden. My rain-gage is fixed on the top of a pole firmly fastened to the end wall, and rising about three feet above it; and no building within about 100 feet of it, nor any trees of sufficient elevation to affect it by eddy winds.

I ascertain the temperature of the air by a Six's thermometer, which has been checked by a very accurate one of Fahrenheit's: it is placed in a northern aspect, about four feet from the ground, and one foot from the wall.

Should it be your wish to receive either an annual, quarterly, or monthly communication on this subject, I propose beginning with the present year, as it was only during a part of the last that I made use of a Six's thermometer, which is essential to get at a true average temperature.

I will inclose my account for January, 1814, for your inspection; it is remarkable for great cold, an unusual quantity of snow, as well as rain, and for a very great depression of the barometer for a few hours only. From this being a first communication, and so large a quantity of rain being stated to have fallen, you may possibly doubt the accuracy of the gage. To do away any such impression, I will quote the quantity measured by the same instrument in the months of May to December, 1813, inclusive:—

May, 1813, 2·5 inches: June, 2·9: July, 2·4: Aug. 1·6: Sept. 3·1; Oct. 6·9: Nov. 3·5: Dec. 3·5.

January 1814.

BAROMETER

Plate XII

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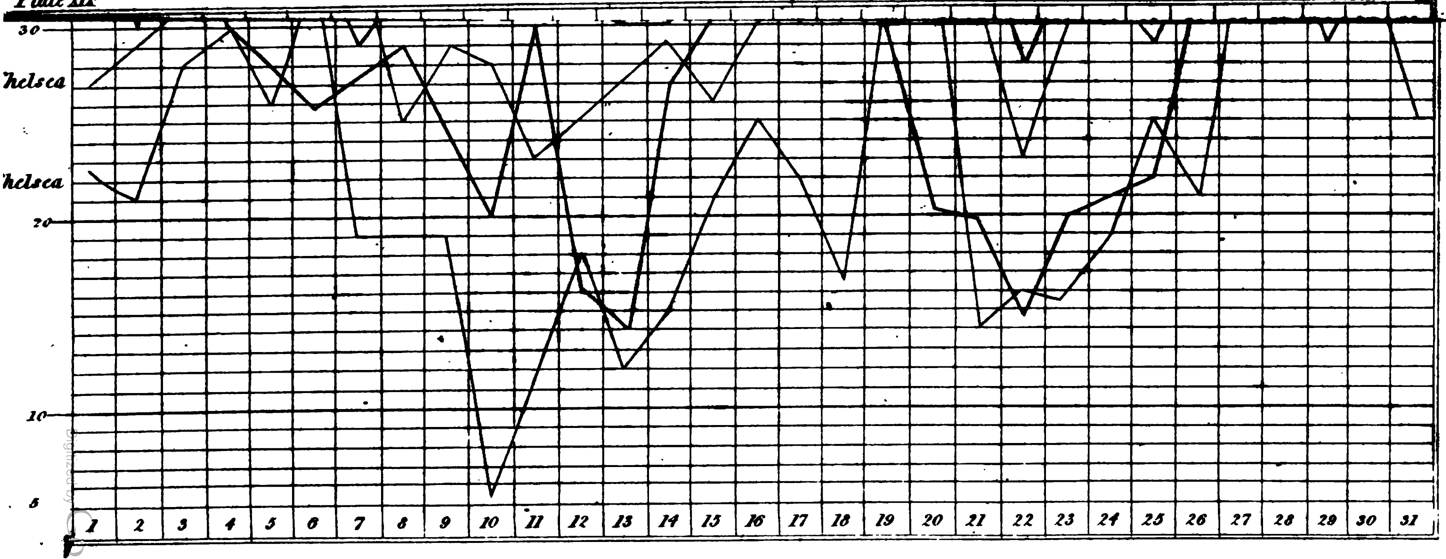

Engraved by J. Shury. for Dr. Thomson's Annals. Published by R. Baldwin. Paternoster Row. April 1.1814.

Editor has added the line of the maximum and minimum height of the thermometer at Chelsea, by way of comparison. The greater height at Plymouth is no doubt owing to the vicinity of the sea.

Winds, state of the Weather, and quantity of Rain.

Date.	*Wind.*	*Rain.*	*Observations.*
1	E	—	Hazy morning; bright day.
2	Var.	—	Fog, and hoar frost.
3	E	0·6	Rain 0·6.
4	E N E	—	Snow showers.
5	N	—	Ditto.
6	N E	—	
7	—	—	Cloudy.
8	—	—	Snow showers.
9	—	—	Bright day.
10	N E to S E	3·2	Ditto morning; snow eve.
11	S E		Snow.
12	N	—	Bright.
13	N E	—	Ditto morning; cloudy eve.
14	E	0·3	Sleet.
15	E		Cloudy, and showers.
16	N E		
17	E		
18	—	3·5	Fog, morn; eve, rain.
19	N E		Morning, rain; hail and snow eve.
20	N	0·5	Snow showers.
21	W N W		
22	N		
23	N N W	—	Bright day.
24	N	—	
25	Var.	—	Cloudy.
26	S to N W	1·3	Showers.
27	Var.		Rain.
28	W	1·8	Fog, heavy showers.
29	W to N W		Ditto, and hail.
30	N W	0·1	Showers.
31	N W	0·1	Snow showers, hail, and sleet.
		11·4[†]	

On the night of the 10th there was a great fall of snow: *average* depth, two feet. A column of it, corresponding to the area of the funnel of the rain-gage, being carefully separated, by removing the surrounding snow yielded when melted 3·2 inches rain.

There fell between the hours of one of the afternoon of the 18th, and nine of the morning of the 19th, 2·8 inches rain; on the afternoon of the same day, 0·7 inches rain, with hail and snow; on the night of the 19th, 4 inches of snow.

N. B. The rain of the 18th and 19th was deep snow at Exeter.

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[*] The upper line in the scale of the thermometer shows the greatest degree of heat indicated by Six's thermometer in each particular day: the lower line, the greatest degree of cold. The observations of the barometer are made twice in each day, at ten A.M. and at four P.M.

[†] May not this enormous quantity be partly owing to drifted snow.—T.

RESULTS.

Prevailing Winds: in the former part of the month, Easterly; in the latter, Northerly and North-West.

Barometer; Greatest height..........30·16 inches;
Least..................28·40
Mean of the period......29·54
Thermometer: Greatest height................48°
Least.........................14
Mean of the period.............31·838
Rain, and snow (when melted)......11·4 inches.

⁂ The Editor has received Mr. Fox's communication with pleasure. Together with Dr. Clarke's, it will make us better acquainted than we are at present with the south coast of England. He conceives that a quarterly or half-yearly report would be more interesting than a monthly one. It would have this additional advantage, that we should be better enabled to give it a place in the *Annals of Philosophy*; and this is a consideration of importance, as we are already at a loss to find room for the valuable communications with which we are favoured; an evil which will not be diminished as the work becomes more generally known.—T.

ARTICLE VII.

Experiments tending to prove that neither Sir Isaac Newton, Dr. Herschel, Sir H. Davy, Mr. Leslie, Sir H. Englefield, nor any other Person, ever decomposed incident or impingent Light into the Prismatic Colours. By Joseph Reade, M.D.

(To Dr. Thomson.)

SIR, Cork, Jan. 24, 1814.

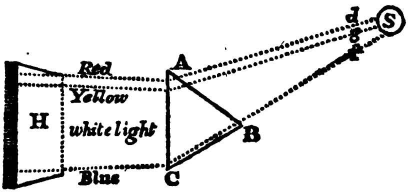
SIR ISAAC NEWTON, for the purpose of decomposing light, made a small hole in his window-shutter $\frac{1}{4}$ of an inch in diameter; and having placed a prism so as to refract and receive a spectrum on a sheet of white paper, perceived seven colours in the following order, viz. red, orange, yellow, green, blue, indigo, and violet; these he supposed to be primary colours, which, when combined in certain proportions, gave white or transparent light. The necessary shortness of a letter will not allow me to enumerate his experiment; I therefore refer to his Optics. That this philosopher was mistaken in supposing he analysed incident light will appear evident from the following experiments and observations. When we look with a prism at a window, the light passes through the panes, and likewise through the prism to the eye, undecomposed, and consequently colourless; but when we look to the frames we perceive an artificial rainbow, of reflected blue, red, and yellow. Any opaque substance, as a piece of black cloth or paper, when pasted on the window, will produce the same effect; and the more dense or dark, the deeper the tints or fringe. The north or top of the paper will be fringed with blue, the south or bottom with red and yellow rays. Now it is evident, if light is decomposed by merely passing through the prism according to the different refrangibilities of its coloured rays, that light admitted through the panes should be equally decomposed with that in the vicinity of the opaque frames. To place this argument in a stronger point of view, I made the following experiment. I cut two holes in my window shutter, one the diameter of $\frac{1}{4}$ of an inch, mentioned by Sir Isaac Newton, the other the diameter of four inches; and having darkened the room, and applied a prism, I found that the small aperture admitted light tinged with the seven colours, which I could receive on a sheet of white paper; the larger orifice was also fringed round with seven prismatic colours, and pencils of white light passed through the centre. Here I must again ask, if white incident light were decomposed by merely passing through the prism, why was not that coming through the centre equally decomposed with that at the edges. And however contrary to received opinion, I am confident it is nevertheless true that incident light has never yet been decomposed, but that all experiments hitherto made have been on light condensed and reflected by opaque substances. If we paste a piece of black cloth on the window, whose colour, as I have shown in my last communication on blackness, (see Philosophical Magazine,) arises from the reflection of condensed rays of blue, red, and yellow, on applying the prism a fringe of red and yellow appears at the south: this does not proceed from a decomposition of incident light striking on the edges of the cloth, but it proceeds from an actual decomposition of the condensed coloured rays of the black cloth itself. The prism decomposes these three primary colours according to the order of their different refrangibilities; and as the red and yellow rays are more refrangible than the blue, as I shall show in my next communication, they are brought down by the prism, and the black cloth remains of a blue colour: the farther we move from the window, the more refrangible the red and yellow rays become, and consequently the decomposition is the greater. In this experiment the north of the cloth reflects blue rays, the south red and yellow, proving in the most satisfactory manner that there are but three primary colours; and as all the secondary or mixed colours can be formed by blue, red, and yellow, to call others into existence would be contrary to the beautiful simplicity of nature, and unnecessary. But it might be asked, if there are but three primary colours, how did Sir Isaac Newton produce a spectrum of seven? The following experiment will explain this. Paste a strip of black cloth or paper six inches by three on the window, on the south you perceive a fringe of reflected red and yellow: paste another similar strip parallel to this, at about four inches distance, on looking through the prism you perceive the north to be fringed with blue: thus we have three primary colours nearly in contact; the yellow rays of the upper paper being the most refrangible come nearest to the blue of the lower paper; and if we approach them, a green is formed by their mixture; so that we can now without any difficulty account for five of Sir Isaac Newton's colours—red, orange, yellow, green, and blue. By making a small hole in his window-shutter, he brought the northern and southern fringes into contact or mixture, and produced five colours with three. It now remains to account for the indigo and violet; and here I must again refer my reader to my last communication, in which I have shown that blackness arises from the reflection of blue, red, and yellow; which being granted, the solution of this otherwise difficult question becomes easy. The red and yellow of the lower cloth or paper being more refrangible than the blue, were brought down by passing through the prism, leaving the upper part of the lower edge (when illuminated by the undecomposed light coming through) blue; under the blue appeared indigo, which, as I shall hereafter show, is composed of blue, red, and yellow, in a different state of condensation from black: and at the bottom of all appeared violet, arising from a great quantity of yellow and red which had been brought down mixed with the black rays. From this experiment we might conclude that Sir Isaac Newton, by mixing three primary colours, made seven.

But I am well aware it might be objected that Sir H. Englefield and others decomposed, or thought they decomposed, incident light coming immediately from the sun, by passing it through a prism placed at an open window: so far, however, from refuting, this experiment confirms my opinion that incident light was never yet decomposed, as I shall now endeavour to prove. The prism being a semitransparent substance, when turned in such a manner on its axis as partly to reflect and partly to transmit the rays of light, (for it will never decompose if turned at right angles to the sun,) condenses and reflects fringes of blue, red, and yellow, from each of its angles; and these fringes of reflected light, being carried forward through the prismatic planes by the incident and undecomposed light, intermix by their different refrangibilities, and form a spectrum of seven colours; and as there are three angles in every prism, so there are two spectra always formed, in the same manner as three strips of paper pasted parallel to one another on the window will also form two spectra. As I am well aware that my experiment and opinions are in opposition to authorities of the first respectability in science, and as I am also certain that science and liberality always go hand in hand, I rest my idea on experimental inquiry. To show that the decomposition of light takes place only at the prismatic angles, and arises entirely from those fringes of reflected light, I made the following experiment. I placed my prism on a table at an open window, through which the sun was shining very powerfully; and having made a spectrum, I slowly turned it on its axis, until I separated the red and yellow from the blue; and in place of green, white light passed through between the angles. I now ascertained that the red and yellow rays passed through the upper angular edge, by intercepting them with my finger placed on it; and by running my finger along the middle angle, I intercepted the blue rays; and by pasting a strip of paper between those two angles, I made two spectra. But to place the fact beyond the possibility of doubt, standing at a little distance I looked, by means of another prism, at the light passing through, and perceived three beautiful fringes hanging from the angles. Indeed it is surprising that those fringes, as I have proved, so evident to the eye, and so highly important in their consequences, should have escaped the observation of such able and accurate experimenters as those already mentioned. I shall conclude this paper with the following deductions.

1. That incident light has never yet been decomposed, and that Sir Isaac Newton and other philosophers only decomposed light reflected from opaque substances, or fringes of blue, red, and yellow.

2. That there are but three primary colours, blue, red, and yellow; by the mixture of which, either by the prism or the painter, all the others are formed.

3. That Herschel, Leslie, Davy, Englefield, and other philosophers, drew their conclusions relative to the heating power of the prismatic colours from erroneous data, viz. from experiments on reflected light, whose heat must in a great measure depend on the reflecting media, and also on the thickness or thinness of those parts of the prism through which the fringes pass: thus the red and yellow rays passing through the very thin upper angle, must be accompanied by more radiant caloric than the blue rays which pass through the thickest. The following diagram will demonstrate my opinions; and as I am at present engaged in a series of experiments to prove that the prismatic coloured rays have similar heating powers, I shall not anticipate.



Let S represent the sun, *d*, *g*, *f*, rays of undecomposed light, impinging on the angles, A, B, C, of the prism; these carry forward the angular fringes, which being refracted towards the perpendicular, fall on the spectrum, H. The red and yellow rays passing through the thin angle, A, must be more heated when falling on the spectrum, H, than the blue rays passing through the angles, B, C.

Sir, I beg leave to remain your obedient servant,

Joseph Reade.

ANNOTATION.

The Editor would recommend his old friend and pupil Dr. Reade to consult the Philosophical Transactions, volumes 7, 8, 9, 10, and 11; in which he will find the controversy between Sir Isaac Newton and Father Pardies, Dr. Hooke, M. N., Mr. Linus, Mr. Gascoigne, and Mr. Lucas; and likewise the optical treatise of Marat, who made so infamous a figure at the beginning of the French Revolution. He will see that most of his opinions are not new, and he will find Newton's refutation of some of the most plausible of them quite satisfactory. I am sorry to add that he will find himself by no means in very respectable company, either in point of knowledge of the subject, or of honesty.

ARTICLE VIII.

Astronomical and Magnetical Observations at Hackney Wick.

By Col. Beaufoy.

Latitude, 51° 32′ 40·3″ North. Longitude West in Time 6″$\frac{82}{100}$.

Feb. 20, Immersion of Jupiter's 1st Satellite	7h	26′	26″	Mean Time at H.W.
	7	26	32·8	Ditto at Greenwich
Feb. 21, Immersion of Jupiter's 3d Satellite	7	32	27	Mean Time at H.W.
	7	32	33	Ditto at Greenwich
Emersion	11	09	30	Mean Time at H.W.
	11	09	36·8	Ditto at Greenwich.
Feb. 27, Emersion of Jupiter's 1st satellite..................	11	35	27	Mean Time at H.W.
	11	36	33·8	Ditto at Greenwich.

N.B. The time of the emersion of Jupiter's 3d satellite on 21st February is nearly 13′ later than the time set down in the Ephemeris.

Magnetical Observations.

1814.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Feb. 18	8h 45′	24° 14′ 03″	1h 50′	24° 20′ 31″		Discontinued from the shortness of the days.
Ditto 19	8 40	24 14 35	1 50	24 23 47		
Ditto 20	8 50	24 14 51	1 55	24 22 05		
Ditto 21	8 52	24 14 51	1 55	24 24 33		
Ditto 22	8 55	24 18 07	— —	24 — —		
Ditto 23	8 45	24 13 02	1 50	24 23 12		
Ditto 24	8 45	24 18 27	1 55	24 22 12		
Ditto 5	8 45	24 15 06	2 00	24 19 32		
Ditto 26	8 50	24 15 18	2 00	24 21 50		
Ditto 27	8 45	24 14 27	1 45	24 21 41		
Ditto 28	8 50	24 14 11	1 50	24 20 57		

Mean of Observations in Feb.	Morning	at	8h	47′..... Variation	24°	14′	50″	West.
	Noon	at	1	52 ..... Ditto	24	20	58	
	Evening	at	—	— ....., Ditto	—	—	—	Not obs.

Ditto in Jan.	Morning	at	8h	52′	.....	Variation	24°	15′	05″	West.
	Noon	at	1	58	.....	Ditto	24	19	03	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
Ditto in Dec.	Morning	at	8	58	.....	Ditto	24	17	21	West.
	Noon	at	1	53	.....	Ditto	24	19	49	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	42	.....	Ditto	24	17	42	West.
	Noon	at	1	54	.....	Ditto	24	20	24	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	.....	Ditto	24	15	41	West.
	Noon	at	1	59	.....	Ditto	24	22	53	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	.....	Ditto	24	15	46	West.
	Noon	at	2	02	.....	Ditto	24	22	32	
	Evening	at	6	03	.....	Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	.....	Ditto	24	15	58	West.
	Noon	at	2	02	.....	Ditto	24	23	32	
	Evening	at	7	05	.....	Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	.....	Ditto	24	14	32	West.
	Noon	at	1	50	.....	Ditto	24	23	04	
	Evening	at	7	08	.....	Ditto	24	13	56	
Ditto in June.	Morning	at	8	30	.....	Ditto	24	12	35	West.
	Noon	at	1	33	.....	Ditto	24	22	17	
	Evening	at	7	04	.....	Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	.....	Ditto	24	12	02	West.
	Noon	at	1	37	.....	Ditto	24	20	54	
	Evening	at	6	14	.....	Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	.....	Ditto	24	09	18	West.
	Noon	at	0	59	.....	Ditto	24	21	12	
	Evening	at	5	46	.....	Ditto	24	15	25	

Magnetical Observations continued.

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
March 2	8h	50′	24°	17′	37″	1h	45′	24°	23′	25″		Discontinued from the shortness of the days.
Ditto 3	8	50	24	15	19	1	55	24	21	50		
Ditto 4	—	—	24	—	—	1	55	24	23	02		
Ditto 5	8	50	24	14	55	1	55	24	23	05		
Ditto 6	8	55	24	15	42	1	50	24	23	11		
Ditto 7	8	55	24	16	19	1	50	24	24	14		
Ditto 8	8	55	24	14	39	1	45	24	23	18		
Ditto 9	8	50	24	14	29	1	45	24	23	43		
Ditto 10	9	05	24	16	03	—	—	—	—	—		
Ditto 11	—	—	—	—	—	2	05	24	22	46		
Ditto 12	8	45	24	14	17	1	55	24	22	32		
Ditto 13	8	55	24	19	33	1	55	24	22	49		
Ditto 14	8	50	24	14	13	1	55	24	23	11		
Ditto 15	8	55	24	15	01	1	50	24	23	33		
Ditto 16	8	55	24	14	51	1	50	24	22	17		
Ditto 17	8	50	24	14	57	1	55	24	20	22		

Rain fallen { Between noon of the 1st Feb. / Between noon of the 1st Mar. } 0·17 inches.

Article IX.

Vindication of the Attack on Don Joseph Rodriguez' Paper in the Philosophical Transactions. By Olinthus Gregory, LL.D. of the Royal Military Academy, Woolwich.

(To Dr. Thomson.)

SIR,

It was not till yesterday, that I saw the thirteenth Number of your *Annals of Philosophy*, or I should have troubled you earlier with a few observations, occasioned by the notice you have in that number thought proper to take of my animadversions on Don Rodriguez' paper, in the Philosophical Transactions for 1812.

Your notice is short, and my remarks shall be as concise as they can be rendered consistently with the nature of your strictures. Indeed, had you enabled your readers to judge correctly on the subject, either by a fair abridgement of my animadversions, or by referring them to N° 159 of Nicholson's Philosophical Journal, or to N° 179 of Tilloch's Philosophical Magazine, for the animadversions themselves, it is highly probable I should have remained silent on so unpleasant a topic.

There are three points in your brief notice, respecting which I beg permission to speak for myself. 1st. "Dr. Gregory has published a letter on the subject, in a style *quite new to astronomical discussions.*" What am I, or what is the public, to understand by this? Is it because the style is *incorrect*, or because the tone is *argumentative* and *decisive*, or because the language is *intemperate*, that you thus express yourself? The first, I apprehend, Dr. Thomson is too courteous and well-bred to think of saying. The second interpretation is equally improbable: for who that has ever heard of the letters and pamphlets which, in the course of the last half century, have appeared in this country, on the subjects of Irwin's marine chair, the equation of time, timekeepers, supposed errors in the Nautical Almanac, &c., will venture to affirm that statement, counter-statement, reasoning, refutation, and even recrimination, are "*quite new* to astronomical discussions?" Is it then, because my language was intemperate? That I deny, and I am persuaded that every impartial judge who reads my letter, above referred to, will say that no such charge can fairly be brought against it. I have, I believe, shown decisively, that every position taken by Don Rodriguez is untenable. I have, as the public generally admit, and as you do not attempt to deny, (if I rightly understand you,) refuted every argument advanced by that gentleman to prove the incorrectness of Colonel Mudge's observations; I have proved that the Don's opinions on the general subject run counter to those of the greatest philosophers in Europe; I have shown that an error of 4½ seconds could not arise from the fixing, the construction, or the use, of the instruments employed; and have shown that, so far from there being an error of that magr tude, there cannot possibly be one of even *half a second,* unl there be a corresponding or greater error, in the series of obser tions *for years* at the fixed observatories of Blenheim and Por mouth. How, then, can any one say, as you do, that " the poi *can only* be settled by repeating the observations," and that " *u* *founded doubts* may remain on the subject?" Or, why should t " style" of my letter be characterized as " *quite new* in astro mical discussions?" May not an astronomer or a mathematician, well as any other man, refute untenable arguments, especially they be uncandidly advanced, and have a tendency to impeach accuracy of a great national work?

2dly. You, however, seem to express your surprise that I sho " affirm that the sole object of Don Rodriguez is to elevate French astronomers and depress the English," since his " pape *remarkable* for its moderation and apparent candour." That i remarkable for its *tameness* might perhaps be affirmed. But I h yet to learn that tameness and moderation are synonymous ter And as to " candour," my language was this: " I shall no hope, be deemed uncandid, if I say, that to me this object *app* to be no other than the depression of English (and perhaps ot ingenuity and exertion, in order to the undue exaltation of French scientific character." But I did not rest satisfied merely hinting my *suspicions* as to the point. I adduced some gular passages from Don Rodriguez to show on what those picions were founded; and I have never yet heard of any Bri mathematicians, except those who were then in the council of Royal Society, and the well known, though anonymous, ma matical writer in the Edinburgh Review, whose inferences a Don Rodriguez' " candour" did not coincide with mine.

3dly. You accuse me of " *insinuating* pretty plainly, that Royal Society *concurred* with Don Rodriguez in his design depreciating English astronomers, for the purpose of unduly e ing others. *This I most positively disclaim.* I know that than two-thirds of the Fellows of the Royal Society scarcely attend its meetings, and have no more to do with its manager or its publications than I have: I could not, therefore, thin imputing any such intention to *them.* Nor did I think of imp such intention to the Right Honourable President or to the cou But the circumstance to which I then adverted as matter of prise, and which I now express *clearly* that I may no longe misunderstood, is, that the mathematical members of the co for 1812 should examine Don Rodriguez' paper, and neither ceive his obvious intention to lower the character of Co Mudge, nor his gross misrepresentation of the history of mea ments of degrees in England, France, and Lapland, nor disc that his arguments were incompatible with the received opinio the best philosophers, at the same time that they were totally conclusive and nugatory.—Here lay, and here still lies, the matter of astonishment.

I am aware that it has been customary to warn the public, that the Royal Society is not responsible for what may appear in its Transactions: and, in reference to the Society as a body, or to a great majority of its members, this is correct enough. But where a discretionary power of adopting and rejecting is exercised, responsibility must attach somewhere. Now the members of the council are appointed for this express purpose, among others. They examine, they set aside, they select, they publish: and the learned world is at liberty to form its judgment of the result.

With regard to mathematical papers, for example, they have in the course of a few years—

Rejected—Professor Vince's paper on the Cause of Gravitation, published since in a separate pamphlet.

Rejected—Professor Lax's paper on Re-entering Angles, published, also, in a separate pamphlet.

Rejected—The explanation of Mr. Barrett's theory and elaborate computations relative to Life Annuities, &c. Afterwards published in a separate pamphlet by Mr. Francis Baily.

Adopted—Don Rodriguez' attempt to prove the inaccuracy of the Trigonometrical Survey of England and Wales; that is, a paper written to show that that important undertaking is conducted by men, who, with instruments of unparalleled accuracy, have *made blunders of* FOUR AND A HALF SECONDS *in a series of zenith distances!*

These examples might, easily, be much extended. But I simply specify the above as a few which subsequent publications have rendered well known, to prove that the council exercise a discretionary power. In which of them they have exercised that power judiciously, in which injudiciously, the other members of the society, and the scientific world in general, may decide without difficulty by a comparative examination of what was rejected and what was retained

I have only to add, that in what I have here said, I am not conscious of being at all actuated by unworthy personal feelings: in proof of which, allow me to state, that I do not recollect the name of a single gentleman who was in the council between the years 1812 and 1813, except that of the late Astronomer Royal; and for his character, both scientific and private, none can cherish a higher esteem than has for many years been entertained by,

Sir, Yours respectfully,
OLINTHUS GREGORY.

Royal Military Academy, Woolwich,
Feb. 8th, 1814.

APPENDIX BY THE EDITOR.

I expressed my opinion of Dr. Gregory's paper, in the *Annals* *of Philosophy* for January, in as mild terms as I could. There is nothing in the present communication to induce me to alter that opinion; on the contrary I consider it as still more exceptionable and improper than the former. As to the tirade against the council of the Royal Society, these gentlemen do not stand in need of any defence from me; nor indeed would it be decorous in me to attempt it. I may just observe that there is no set of men more apt to indulge in inaccurate or absurd views than mathematicians. If this position stood in need of confirmation, it would be easy to bring forward some of the most celebrated names both of former and present times in confirmation of it. If a mathematician presents an absurd or trifling paper to the Royal Society, surely it is not incumbent on that learned body to print it, merely because it is on a mathematical subject. Of the three papers mentioned by Dr. G. as rejected by the Royal Society, I have seen only one, and I must say that, as far as I understand the subject, it appears to me to be no demonstration at all. I have even seen a demonstration (by Mr. Playfair) of what the author of that paper endeavours to refute, which I consider as conclusive. Surely it would have been very indecorous in the Royal Society to have published, in their Transactions, a false refutation of an hypothesis of Sir Isaac Newton. As to Colonel Mudge's Trigonometrical Survey, it constitutes two quarto volumes. It could not therefore have found a place in the Transactions; but must have been published apart. Surely it was unreasonable, on the part of the Board of Ordnance, to expect that the Royal Society, whose funds are derived from the contributions of its members, and which is far from rich, should be at the expense of publishing the results of a national undertaking. It was the business of government to publish the book, and accordingly this was done by the Board of Ordnance. The British public are well aware, that the Royal Society stands quite in a different predicament from the French Institute, or the Academy of Sciences of Berlin and Petersburgh. The members of these institutions are pensioned by Government, and all the expenses of their publications defrayed by the public. Whereas in Britain all this is done at the private expense of the individuals forming the association. Surely they are at least entitled to refuse the additional burden of publishing works undertaken by Government for the good of the nation in general.

Nor is Dr. Gregory more correct in his list of mathematical papers published in the Philosophical Transactions. Though the papers of Mr. Vince, Professor Lax, Mr. Barrett, Dr. Gregory, and others were refused admittance, for reasons into the validity of which it is not my business to inquire, still the volumes of the Transactions contain some of the most important mathematical papers that have been published for many years in any country of Europe. I shall give the list of them for two years, 1811 and 1812, (the only volumes that happen to be at hand,) merely to show the reader how very partial and inaccurate Dr. Gregory's statement is.

1. On the Rectification of the Hyperbola by means of two Ellipses; proving that method to be circuitous, and such as requires much more calculation than is requisite by an appropriate theorem; in which process a new theorem for the rectification of that curve is discovered. To which are added some farther observations on the rectification of the hyperbola: among which the great advantage of descending series over ascending series in many cases is clearly shown; and several methods are given for computing the constant quantity by which those series differ from each other. By the Rev. John Hellins, B.D. F.R.S., and Vicar of Potter's Pury in Northamptonshire.

2. On the Solar Eclipse which is said to have been predicted by Thales. By Francis Baily, Esq.

3. Account of a Lithological Survey of Schehallien, made in order to determine the specific gravity of the rocks which compose that mountain. By John Playfair, Esq. F.R.S.

4. On the Grounds of the Method which Laplace has given in the second Chapter of the third Book of his Mecanique Celeste for computing the Attractions of Spheroids of every Description. By James Ivory, A. M.

5. On the Attractions of an extensive Class of Spheroids. By J. Ivory, A. M.

6. On the Attraction of such Solids as are terminated by Planes; and of Solids of greatest Attraction. By Thomas Knight, Esq.

7. Of the Penetration of a Hemisphere by an indefinite Number of equal and similar Cylinders. By Thomas Knight, Esq.

8. Observations on the Measurement of three Degrees of the Meridian, conducted in England by Lieut. Col. Mudge. By Don Joseph Rodriguez.

How came Dr. Gregory in the preceding letter to omit mentioning the four other mathematical papers contained in the same volume of the Transactions in which Don Rodriguez' paper is printed?*

ARTICLE X.

Reply to Mr. Allan's additional Observations on Transition Rocks.

By James Grierson, M.D.

(To Dr. Thomson.)

SIR,

I PERCEIVE, in the *Annals of Philosophy* for this month, an answer from Mr. Allan to my observations on transition rocks, which were published in your Number for August last; and in offering a few words in reply, which I think it necessary to do, I shall in the first place state as clearly as I can the point or points mainly at issue betwixt Mr. Allan and me.

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* As it never was intended, by the Editor, that the *Annals of Philosophy* should be made a vehicle for controversy, it is thought necessary to state here that no farther communication on this subject can be admitted.

The object of his paper, entitled "Remarks on the Transition Rocks of Werner," was, I conceive, chiefly to establish the following propositions. First, "that the killas of Cornwall, (that is, according to him, the rock which lies *immediately* on the granite of that county,) belongs to the transition series of Werner." Secondly, "that the granite of Cornwall is possessed of every character by which the oldest varieties are distinguished." And thirdly, that "that granite which forms the nucleus round which Werner conceives all other rocks were deposited," (that is, the first granite formation,) "is in some cases actually of a later date than the transition series." The object of my paper again was to show that Mr. Allan had failed in proving his point; and that the arguments he employed for the purpose would not bear the test of examination. I showed that he had mistaken or mistated the writings of Professor Jameson, and had not reasoned correctly either from his own observations, or from those made by others. Let us now see whether Mr. Allan in his answer has said any thing to invalidate what I maintained. In page 275 of his Elements of Geognosy, Professor Jameson has the following sentence: "Molybdena, menachine, tin, scheele, cerium, tantalum, uran, chrome, and bismuth, are metals of the oldest primitive formation, and only feeble traces of them are to be observed in newer periods." This sentence Mr. Allan quotes to prove that, according to Professor Jameson, tin is to be found only in the oldest primitive rocks. Such, however, is certainly not the Professor's meaning. Oldest primitive *formation* does not here signify the oldest primitive *rock*. It signifies the oldest repository of the metal. And every body knows that a metal may occur in *veins* even in the oldest granite, and yet be newer than when *disseminated* through clay-slate.

In page 261 of his Elements, Professor Jameson says of tin, that "it occurs in very old veins that traverse granite, gneiss, mica-slate, and clay-slate." Mr. Allan asks me, "whether I suppose that this granite, connected with gneiss, is that belonging to the newest formation, alluded to in a subsequent paragraph of the same page." My answer is that I do; and for the following reasons. The Professor says: "tin is of nearly contemporaneous origin with old primitive rocks. Thus it occurs in very old veins that traverse granite, gneiss, mica-slate, and clay-slate." Had he meant the oldest granite, he would have said not *old* but the *oldest* primitive rocks. And that he did not mean the oldest granite is evident from the next paragraph, where he says, that "tin occurs disseminated through granite, and in beds that alternate with strata of granite;" and that "this granite appears to belong to the *newest* formation." As to the connexion of this granite with gneiss, alluded to by Mr. Allan, I beg leave to refer him to Professor

Jameson's third volume, p. 108, where he will find it described as resting on gneiss.

Mr. Allan refers back to Professor Jameson's second volume, published in 1805, where, speaking of tin, the Professor says: "it occurs only in primitive rocks as granite, gneiss, &c.; and in the oldest of all the metals." Such, no doubt, was his opinion in 1805; but subsequent observations must have induced him to alter it: and in 1808, when his third volume was published, to consider tin as occurring not only in primitive but also in transition rocks. Thus then it appears that the *newest* granite is that which contains the tin and wolfram. I do not "reduce the age of these to the period of the transition series;" I only say that tin and wolfram occur in granite, gneiss, mica-slate, and clay-slate, and sometimes in transition rocks; and that the granite is of the newest formation. But even allowing that these metals occurred in the oldest as well as in the newest granite, nothing would thereby be added to the strength of Mr. Allan's argument. For if they are found in both kinds of granite, their presence in that of Cornwall can never prove it to be the oldest. Neither can this circumstance of itself prove it to be the newest. But Mr. Allan admits that the Cornwall granite contains fragments. Now the essential character of new granite is its containing fragments. Mr. Allan seems to think that I was not entitled to put the question, "Who ever heard of fragments being found in the first granite formation?" and replies, "All I have in answer to say is, that the granite of Cornwall presenting the characters of the most ancient cavities of that rock, according to the authority of Werner himself, as given by Professor Jameson, does contain, in the veins which extend from it, abundance of fragments." I answer that it has then the character of the newest, not of the oldest granite; for this Cornish rock, it seems, contains tin and wolfram along with fragments; and these two circumstances *conjoined*, characterize the newest granite.

Mr. Allan's opinion respecting conglomerated rocks signifies but little. The fact is that according to Werner such rocks do occur in primitive country. Mr. Allan may call them by a different name if he chooses.

The only example he produces of the killas, as he chooses to call it, resting immediately on the granite, is at St. Michael's Mount; and there we are told by him, that it is a fine-grained gneiss, or at least, has all the "appearance" of it. Now in the account of Werner's transition rocks, as given by Professor Jameson, no such rock as this is mentioned. Fine-grained gneiss is not among the number. And hence I repeat that I am, in fair inference, still entitled to maintain, that the killas of St. Michael's Mount, the only variety seen immediately resting on the granite by Mr. Allan, belongs not to the transition series of Werner, as given by Professor Jameson, and referred to by Mr. Allan. He has not therefore made out his first position. Neither can we admit the second;

for the granite of Cornwall contains fragments; and this, as has been shown, is according to Werner, distinctive of the newest granite. We are, in consequence of all this, destitute of any proof of the third proposition of Mr. Allan, namely, that "the granite which forms the nucleus round which all the other rocks were deposited," (*that is the first granite formation*) "is actually of a later date than the transition series."

Mr. Allan in his former paper asserted, that "floetz rocks are never found conformable with the transition rocks." He was called upon by me for his proof of this, p. 106 of your August Number. But he has thought proper to decline making any reply. He still withholds his proof.

I likewise objected to a statement of his respecting granite veins; which he says, according to the Wernerian geognosy, occur only in such rocks as are composed of the same constituents, such as gneiss and mica-slate; and I mentioned that I had never, for my part at least, seen or heard of any writer on Werner's system who made such an assertion. On the contrary, Professor Jameson, in his third volume p. 107, states that granite veins traverse, not only gneiss and mica-slate, but clay-slate: a rock not composed of the same constituents with granite, and every body knows that mica-slate itself wants one of them. Mr. Allan in his answer takes no notice of this matter. I wished for his reason, but he has produced none. Perhaps he is like a celebrated character in the play, unwilling, though he may have plenty of reasons, to give me one on compulsion.

I now leave you and your readers to judge whether I was not justifiable in imputing to this gentleman inaccuracies or mis-statements. But I find that I am not the only person who has had reason to complain of him in this respect. I observe in the Philosophical Magazine for December last, p. 429, a paper by J. A. De Luc, Esq. F.R.S., entitled, On the Phenomena of St. Michael's Mount in Cornwall, in which he is at great pains to shew that Mr. Allan, in his Remarks on the Transition Rocks of Werner, has given a very erroneous account of his observations. De Luc says that Mr. Allan must never have seen his book at all; but must have contented himself with an account of it by some inattentive reviewer. "If," says De Luc, "Mr. Allan had seen my own work, he would not have thought I was *mistaken*; since I have not only described, myself, the very phenomena that he opposes to me, as observed in the specimen which he laid before the Edinburgh Society, but this is only a very small part of the geological circumstances which I have described in that country. The principal of them against the Huttonian system were: the stratification of granite, the broken and shattered state of its strata, no less than those of killas by the same cause; that of the subsidence of part of them; and the evidence of the veins of St. Michael's Mount having all the characters of mineral veins, on which the Huttonian system had spread so many errors."

In page 433, De Luc complains of Mr. Allan for a partial statement of his doctrine. His words are: "Those," says Mr. Allan, "who were inclined to consider this rock as an original deposite, have accounted for its formation in different ways." "I think," adds De Luc, "that as Mr. Allan mentions me for a part of the subject, he ought to have expressed not only the way that I have explained the production of granite, but the proof which he might have found in my works." M. De Luc's paper consists of ten pages, and the greater part of it is occupied in correcting the inaccuracies of Mr. Allan.

Mr. Allan very politely thanks you, sir, for the confirmation you have given to his opinions, in your very clear and distinct account of your late observations in Cornwall. He seems anxious to make it appear that you are on his side. But it is quite clear to me that what you say gives no sort of countenance to his doctrine. He maintains that the granite of St. Michael's mount must be held to be primitive, or part of the nucleus round which Werner conceives all other rocks were deposited. You, on the contrary, have shewn that there exists at that place irresistible proof of the granite there not being part of the above nucleus; and very truly observe that had Mr. Allan "given his reasoning powers fair play, he would, from his own observations, have come to the same conclusion." The granite of St. Michael's Mount is either primitive, or it is not. If it be primitive, Mr. Allan's statements, as I have already shewn, demonstrate it to be connected with gneiss, and not with transition rocks. And if it be not primitive, its connection with transition rocks forms no objection to the system of Werner. It only shows that the principles of that system are purely inductive, and readily accommodate themselves to the progress of discovery. Mr. Allan tells us that he argues against Werner and not against his pupils. But as you well observe, "it is not doing justice to Werner to compare the opinions which he held 13 years ago with our present knowledge." If Mr. Allan does not argue against the Wernerian doctrines as they *at present* appear in the best writers on the subject, his arguments are frivolous. He might as well set himself to overthrow the system of Burnet or of Buffon. You have also shewn that he was innaccurate in the account which he gave of the application of the word killas.

Mr. Allan tells us that "he is satisfied that the sentiments contained in my paper are not all, originally at least, my own;" and one of his proofs is that "some of these sentiments had long before reached him from another quarter." Proof positive, no doubt! Had Mr. Allan the humility to imagine that his paper was not calculated to excite some attention, among the other "men of science" of this place, interested in the same sort of speculations with himself? I do most readily give him credit for every good quality; but really this is more than I can bring myself to admit. It would be a degree of humility, indeed, which most people, I believe, would not be disposed to look upon as any recommendation; and therefore I must think that Mr. Allan expected his paper would be talked of, and its merits discussed, among the mineralogists of Edinburgh. But if this was the case, can he be surprised that 'sentiments' (I suppose by sentiments in this place he means objections to his paper), which are obvious to every attentive reader who understands the subject, should have reached him from more quarters than one. What would Mr. Allan think of the state of my intellects, were I to argue that he could not be the sole author of the answer to me, which bears his name, because some of the sentiments it contains have long since reached me from another quarter? I have however, notwithstanding this, no doubt at all of his being "the only one engaged in writing the answer which bears his name." In the concluding words of his own postscript,—"It speaks for itself."

I wish Mr. Allan had told us what quarter he alludes to. He deals in dark and oracular speeches:—

Aio te, Æacida, Romanos vincere posse,

is scarcely more ambiguous than some of his expressions. As to his assertion that I am "not the only one who was engaged in drawing up the communication which bears my name," he must permit me to tell him that he is grievously mistaken, and ought, in his own words, "to feel ashamed" of having said so. I do not know whom he alludes to as having been engaged along with me; but if he does allude, as some tell me he does, to Professor Jameson, I must beg leave to decline so high a compliment. That any of my poor performances should have seemed to Mr. Allan to indicate the hand of so great a master, is what I could never have dreamt of. Surely the occurrence of some of the Professor's ideas in my paper will not prove him to have been engaged in the writing of it. Nothing certainly was more likely to happen than that I should have adopted more or less the sentiments of the master under whom I studied.

With regard to the second proof adduced by Mr. Allan, to show that I was not the sole author of the communication bearing my name; viz., that "you had been pressed by some gentlemen in this place to insert his paper," your answer to that appears to me most satisfactory, and certainly as you state, I was not consulted by you, nor could I possibly know that you had such a paper in your possession.

Mr. Allan favours us, at the beginning of his answer, with a statement of the motives which may induce "men of science" to publish their works, and talks feelingly of "the critic unjustly garbling and interpolating the works of an unfortunate author." The critic, it seems, often shews "acrimony" and "mis-states facts merely to give weight to his own argument." I heartily agree with Mr. Allan in this sentiment. It is true, most true, as many of us unfortunate authors can, to our cost, bear witness. But what is the object of this learned preface? *Necne non erat his locus.*

Or am I the garbling and interpolating critic, and Mr. Allan the unfortunate author? Perhaps this is his meaning. If so, few readers will, I believe, be of opinion that the first two epithets are well applied, whatever they may think of the third.

Mr. Allan intended at one time, it seems, "to have overlooked me entirely." A most prudent resolution certainly, and one which every wise man will not only form, but *adhere to*, whenever he finds himself pressed by an unanswerable argument. He says his paper was not "in the slightest degree shaken by my observations." But I think I have proved that it was shaken, yea, shaken to very tatters; and that nothing he has advanced in answer to me has been able to put it to rights again.

He says that I have "charged him with wilful misconception, with ignorance and presumption; language which will produce a very different effect from that intended by the writer." The fact is that the writer intended to produce no effect whatever, except to shew that Mr. Allan, like many other philosophers before him, had fallen into mistakes, mis-quoted authors, (remember I never said wilfully), and drawn hasty or erroneous conclusions. This I hope was not too much. I do not suppose Mr. Allan disputes the truth of the common adage, *Humanum est errare*. He wrongs me by saying I charge him with "wilful misconception." I only charge him with inaccuracy or mis-statement, meaning by the latter term not wilful mis-statement, or I should have said so, (for I believe, according to the best authorities, the term does not necessarily imply wilfulness), but mis-statement from inadvertency, or at most from prejudice. And the politest disputants, I think, readily represent their opponents as under the influence of this delusion, without its being deemed offensive. As to ignorance, the term certainly does not once occur in the course of my paper; but no doubt every man who attempts to prove that another has reasoned incorrectly, or is anyhow in an error, so far charges him with ignorance. With respect to presumption, though that term is not mentioned neither, yet I must in some measure plead guilty, for I said I should have been better pleased with Mr. Allan's paper if it had been written in a less assuming tone, by which I meant no more than to state, that I thought his style too confident, when disputing against a man who is allowed by all, even by Mr. Allan himself, to be in a great measure, the father of the science of mineralogy.

Mr. Allan complains of the unpleasantness of his task in "being forced to detect the inconsistencies and contradictions" of Professor Jameson. But I trust I have made it sufficiently appear, in a former part of this letter, that he might have spared himself that trouble, as the inconsistencies and contradictions which he attributes to the Professor, exist only in his own misapprehension. He uses these words:—"It is by no means a pleasing task to me to be forced to detect the inconsistencies and contradictions of any one, far less those of a gentleman who I believe has exerted himself, as far as he was able, to promote the study to which we are equally devoted." Now Mr. Allan will pardon me for saying so, but I do think there is here something like a want of decorum;—something like an attempt to under-rate the exertions and merits of Professor Jameson as a mineralogist. But who that knows him, who that knows any thing of the science, is not aware that his writings and labours of various other descriptions, have been of incalculable benefit to it in this country. It will be allowed by most people, I believe, that he is in a great degree the founder of mineralogical science among us

After the very trite observation, "that it is much easier to find defects in a system than to invent a better one," Mr. Allan speaks of "incontrovertible points," and of "descanting solely on their weaker neighbours." He would have us admit, I suppose, that the great and leading principles of the Huttonian theory are incontrovertible points, and that it only fails in accounting for some comparatively unimportant phenomena. Surely a man of Mr. Allan's proficiency in mineralogical science cannot but know that the very first principles of that hypothesis are controvertible points, and have been, not only controverted, but in the opinion of perhaps nine-tenths of the mineralogists of Europe, (whatever they be in his) proved to be false. A philosopher, like Mr. Allan's master, should have known the rules of philosophizing, and Mr. Allan himself knows very well that two of these rules are,—first, to be certain that the cause we assign exists;—and secondly, that if it do exist, it is sufficient to explain the phenomena we ascribe to it. The Huttonian hypothesis is deficient in both these respects. Mr. Allan might have spared himself the pains of the common-place observations he makes about "descanting solely on weaker neighbours," acute arguments of the critic," and "fair and candid discussion." If he means to say that I have descanted solely on the weaker neighbours of his incontrovertible points, I can with truth return him the compliment. If I set him the examble of such conduct he has most faithfully copied it; with this difference only, that after all his descanting on what he may conceive to be my weaker arguments, he has not been able to refute a single one of them.

With respect to the comparison instituted by me betwixt Hutton and Werner, which Mr. Allan says "was quite uncalled for," I beg leave to differ from him in opinion. I think it was called for, and my reason is this.—Mr. Allan had objected to the system of Werner, (among other things no doubt) "that he had drawn conclusions more general than were warranted by the circumscribed field to which he was confined. Now it appeared to me to be a fit reply to Mr. Allan, an *argumentum ad verecundiam*, at least, which should have imposed silence upon him, to shew him that the founder of his own system was liable to the same objection which he brought against the founder of mine, and even in a stronger degree. But I, at the same time, stated that this is not the proper test of the merits of either system, but that we must try them by a standard totally different. What that is I need not repeat.

Mr. Allan "frankly tells" me his opinion respecting the merits of the "two great men," Hutton and Werner. And here I am again under the disagreeable necessity of differing from him. I do not think the hypothesis of Hutton is "founded on observation and pure philosophic induction;" I look upon it as a *fanciful* "contrivance totally unauthorized by all the great," as well as the small "features in nature." He says I ought to know that the phenomena of nature are "totally and entirely" (I suppose these two words mean much the same thing) "irreconcilable to the theory of Werner: so much so, indeed, that my own master, the pupil of Werner, has, as a sacrifice due to common sense, been compelled to introduce many alterations in the system he was taught at Freyberg." Mr. Allan's sagacity ought to have taught him that the principles of Werner are, as I said before, founded on induction, and must therefore appear under a different aspect, in proportion as discovery advances. Professor Jameson therefore, to whom Mr. Allan doubtless alludes in this sentence, has not been compelled to introduce "alterations as a sacrifice to common sense." He has only, like a man, not merely of common sense but of uncommon genius for, and attainments in, his science, applied the Wernerian principles of arrangement to the discoveries that have been lately made. Your observations in a note on Mr. Allan's paper, (p. 111), appears to me unquestionably just. For I consider the systems of Hutton and Werner as standing to one another, in much the same relation as the astronomical systems of Des Cartes and Newton.

Mr. Allan assures me "he never will intentionally mis-state any fact, and that he is not very likely to volunteer his opinion on a subject he does not comprehend." With regard to the first clause of this sentence, I do most sincerely believe him. I never once said nor thought, as I formerly declared, that he would "intentionally mis-state a fact. But with respect to the last clause, I confess my faith is not so firmly fixed. I do not perceive decisive proof of the proposition in the geological works of Mr. Allan. He would have me "feel ashamed" of saying that he laid stress on the opinions of the vulgar; and tells me that "the Cornish miners are a set of people much beyond the class in which I seem desirous to include them." I never doubted that the Cornish miners are a most respectable and intelligent "set of people," nor had the slightest idea of throwing any disparagement upon them. But Mr. Allan himself calls them "the common people," and yet I appeal to every reader whether he does not bring in their opinion in confirmation of his own upon a matter of science. If Shame then have any thing to do with him and me, I think she ought to fix her place of rest on his countenance rather than on mine.

After all, sir, why this mighty bustle by Mr. Allan about his geological labours in Cornwall! One would be tempted to think, from the importance he seems to attach to them, that he had been able to tell us a great deal which we did not previously know concerning that quarter. But is there any thing in his paper to justify such expectation? He informs us of little that I can perceive, except the occurrence of granite veins at St. Michael's Mount, of fragments in the granite there, and of the occurrence of a rock immediately incumbent on this granite different from the ordinary transition rocks of the district. All this, however, we had previously learnt from Professor Playfair and Dr. Berger.

Mr. Allan says that I am spell-bound. It may be so; for I believe, one of the most untoward symptoms of this disease called spell-binding is, that the patient so affected is quite unconscious of his disorder, and can never be persuaded that there is any thing at all the matter with him. But then how is Mr. Allan sure that he is not spell-bound himself? And if so, who knows how long both he and I may remain in this deplorable condition? We shall, however, hope for the interference of some kind magician to break the spell. On looking at me Mr. Allan sees nothing but a wretched captive loaded with "fetters," and fixed like the fair Æthiopian princess of old to a transition rock. Now I do declare that to my visual organs he appears to be exactly in the same situation. What can this arise from but the above-mentioned terrible disease of spell-binding on both sides? He says I will never persuade him, that any thing else than spell-binding "will account for a man gravely teaching the aqueous formation of pumice and obsidian." I again am of opinion that nothing less than this same malady will account for a man gravely teaching the igneous formation of stratified metallic veins, of veins of clay-slate, or of a more fusible substance crystallized in a less fusible one, such as crystals of silver shooting into quartz; or that substances soluble in water, as rock salt and gypsum, were mechanically deposited at the bottom of the sea.

Mr. Allan acknowledges that "he could *see* no line of division at the Louran between the altered rock," as he terms it, "and the common greywacke." He does not, however, think that on this account I ought to imagine that he "had reason to conclude that there was none;" and "begs leave to differ from me, and to assure me, that he is more inclined to believe *his own eyes*, than any other species of demonstration that can possibly be offered." Now, sir, I wish there may not be something of spell-binding here again; for on what principle a man should believe his own eyes when he does not see, I am unable to determine.

I must now take my leave of Mr. Allan, wishing every success to his future geological researches, and repeating, as I retire, his own most impressive words :—" Next time I would recommend a little more consideration." I am very respectfully, Sir,

Your most obedient Servant,
JAMES GRIERSON.

James-Square, Edinburgh,
12th Feb. 1814.

ARTICLE XI.

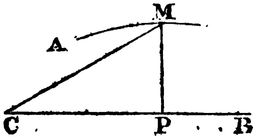
ANALYSES OF BOOKS.

Memoires de l'Academie Imperiale des Sciences de St. Petersbourg. Tomes i. ii. iii. St. Petersbourg. 1809, 1810, 1811.

(*Continued from* p. 225.)

VOL. II.

1. Solution of a Problem, memorable on account of a singular artifice in the calculus. By L. Euler. P. 1. The problem is as follows:—To find a curve line, A M, having the co-ordinates C P = x, P M = y, the arch A M = y S, and the straight line C M = $\sqrt{x^2 + y^2} = z$; in which the integral $\surd v\, \dot{s}$ may be a maximum or minimum, v being any function of z.



2. A simpler Solution of the Diophantine Problem, respecting a Triangle in which the straight Lines bisecting the opposite Sides from the Angles may be expressed rationally. By L. Euler. P. 10.

3. A simple Solution of the following Problem: To find a sphere which will touch four spheres in what way soever they are placed. By L. Euler. P. 17.

4. Of innumerable Curves capable of being described round a fixed Point so that any Angle formed in that Point may cut off from them equal arches. By Nicolaus Fuss. P. 29.

5. Some Observations concerning the Resolution of circular Arches. By Nicolaus Fuss. P. 48.

6. On the Reduction of Expressions of the form $\sqrt[3]{a \pm b \surd c}$ to the binomial $m \pm n \surd c$. By C. F. Kausler. P. 64.

7. Of the curvature of Lines described on the Surface of a Sphere. By Nicolaus Fuss. P. 73.

8. Integration of the Formulas $\frac{\dot{z}\,(1 - z\,z)^2}{(1 + z\,z)\ \sqrt[4]{(1 + 6\,z\,z + z^4)^3}}$ and $\frac{\dot{z}\,(1 + z\,z)^2}{(1 - z\,z)\ \sqrt[4]{(1 - 6\,z\,z + z^4)^3}}$. By St. Rumovski. P. 84.

9. Reflections on those periodical continued Fractions which

express the square Roots of whole Numbers; and on their use in researches into the Factors of Numbers. By C. F. Kausler. P. 95.

10. Demonstration of an Algebraic Theorem. By F. T. Schubert. P. 124. This is a theorem given by Newton in his Universal Arithmetic, without demonstration, under the article Transmutation of Equations. It is as follows:—Suppose an equation of the nth degree of this form (A) $o = x^n - a_1 x^{n-1} - a_2 x^{n-2} - \dots - a_m x^{n-m} - \dots - a_r x^{n-r} - \dots - a_{n-1} x - a_n$; the roots of which are $a, b, c, \dots m, n$; and let the sum of all the nth roots $a + b + c + \dots + m + n = R$, and the sum of all their squares $a^2 + b^2 + \dots + n^2 = R_2$; in like manner let the sum of the rth power of all these roots $a^r + b^2 + \dots n^r = R$, then (B) $R_r = a_1 R_{r-1} + a_2 R_{2-2} + \dots + a_m R_{r-m} + \dots + a_{r-1} R_1 + r . a_r$.

11. Determination of the Radius of Osculation in Lines of Double Curvature. By S. Gouriev. P. 130.

12. A Dissertation on the progressive Motion of Bodies, both free through an indeterminate Space, and not free but confined to the Surface of Curves. By S. Gouriev. P. 138.

13. Remarks on some Equations of the Moon. By F. T. Schubert. P. 171.

14. Calculus of the Oppositions of Uranus and Saturn, observed at St. Petersburgh in 1808. By F. T. Schubert. P. 187.

15. Calculus of the Observations of the great Comet of 1807, made at the Observatory of St. Petersburgh. By F. T. Schubert. P. 213.

16. Meteorological Observations made at St. Petersburgh during the Years 1801 and 1802, by the late Mr. Inochodzow. Arranged by Basil Petrow. P. 224.

17. Astronomical Observations, made at the Observatory of Mitau, in the Government of Courland. By Guill. Theoph. Fred. Beitler. P. 248.

18. On the Genus *Muscicapa*, in the order of *Passeres*. By N. Ozeretskovsky. P. 279. This is a general paper, in which the author points out various defects in the arrangement of this genus, and that several birds are placed in it that do not possess the characters by which it is distinguished.

19. A Description of twenty Species of Grasses not generally known. By M. Sprengel. P. 287. The grasses described, and many of them figured, in this paper, are the following:—

Muhlenbergia erecta,
Muhlenbergia diffusa,
Digitoria pilsosa,
Panicum dichotomum,
Panicum laxiflorum,
Panicum virgatum,
Panicum clandestinum,
Aristida dichotoma,
Agrostis tremula,
Agrostis cinna,
Limnetis cynosuroides,
Aira pensylvanica,
Arundo pygmæa,
Rottböllia muricata,
Poa racemosa,
Poa sudetica,

Poa cœspitosa,	Apluda aristata,
Poa brizoides,	Andropogon cymbarius.

20. Commentary on the Genus of Plants called Ziziphora. By H. Rudolphus. P. 307. In this continuation of his paper from the preceding volume the author gives a description and figures of the species of this genus. They are three in number; namely, ziziphora capitata, z. clinopodioides, and z. serpyllacea.

21. On a Deviation from the usual Position of the Arch of the Aorta, and an unusual Origin of one of its Branches. By P. Zagorsky. P. 318.

22. A Description of some new Minerals. By Alex. Schlegelmilch. P. 321. Four minerals are described in this paper. 1. The iberite, of which a description has been already given in the *Annals of Philosophy*, vol. iii. p. 152. 2. Granular basalt. This is a variety of basalt in granular distinct concretions, which constitutes hills in Georgia and in Upper Armenia. 3. Opalising (*chatoyante*) obsidian. It occurs, likewise, in Georgia. 4. Oxide of chromium. Colour, grass green. It occurs superficial, disseminated, and in veins. Internal lustre, dull, or slightly glimmering sometimes. Fracture, fine earthy, sometimes compact, and sometimes fine splintery. Opake; but the varieties with the splintery fracture are translucent on the edges. Soft. Does not stain the fingers. Streak, whitish. Before the blow-pipe it loses its colour, is infusible without addition, and with borax melts into a fine green glass. It is found at the southern extremity of the Oural mountains, accompanied with chromate of iron.

23. Description of a new Species of Azalea. By M. F. Adams. P. 332. The species described is the azalea fragrans which grows in the northern part of Siberia upon the borders of the frozen ocean.

24. Description of the Camtschatka Fish called by the Russians *Terpice* and *Wachnja*. By M. Tilesius. P. 335. The first of these fish constitutes a new genus, which Stellerus called hexagrammos, and Pallas, labrax. The second seems allied to the genus *gadus*. Tilesius gives it the name of wachnia, and describes two species. The paper is a very elaborate one, and contains a very full description of these fish, together with anatomical observations on their structure.

25. Observations on a Fish called improperly *Herring*. By N. Ozeretskovsky. P. 376. This is a fish found in the great lake Pereslavle Zaleski, and nowhere else. The author shows that it is not a herring, being distinguished from that fish by having eight fins, while the herring has but seven. It is a species of *salmo*. It belongs to the coregoni, or those that are destitute of teeth. He conceives that it may be only a variety of the salmo moræna, brought into this lake from a distance, and altered somewhat in its appearance.

26. On the Labraces, a new Genus of Fish of the Eastern Ocean. By P. S. Pallas. P. 382. This is the genus of fishes described by Tilesius in the 24th paper of this volume. Pallas gives a description and figures of no fewer than six species, one of which is the hexagrammos of Tilesius.

27. Mineralogical Observations made in the Government of Twer. By B. Severguine. P. 399. The whole of this country is a vast plain of sand, clay, or calcareous tuff, over which are scattered rolled blocks of primitive rocks. Many of them possess the characters of the granitic rocks in Finland.

28. On the Theories of Value hitherto established by Writers of Political Economy. By Henry Storch. P. 413. In this, and three other elaborate dissertations that follow it, the author refutes the notion of value entertained by the economists, by Dr. Smith, and by Lord Lauderdale; and endeavours to prove that it is founded solely upon *opinion*.

29. On the Chemical Knowledge of the Chinese in the eighth Century. By Julius Von Klaproth. P. 476. It appears, from M. Klaproth's extracts from a Chinese book written in the year of our era 756, that the Chinese had some faint notions of oxygen. They called it the impure part of air, and said that it combined with sulphur, charcoal, and metals; that it may be extracted from saltpetre by means of heat, and from the black stone called Hhe-tann-ché. They seem to have thought likewise that it was a constituent of water. The notions respecting the metals, as appears from the quotation of Klaproth, are pretty similar to those entertained by the alchymists.

30. A Statistical Description of the Rock Salt and Salt Springs of Russia. By C. T. Hermann. P. 485. Two mines of rock salt exist in Russia. 1. The mine of Iletzk, on the banks of the little river Ilek, which runs in the steppe beyond the Oural. 2. The mine Tschaptschatschi, on the left side of the Wolga, in the steppe of the Oural. This last has never been wrought, the country being uninhabitable for want of water and wood. The principal salt springs in Russia are those of Perme, which furnish a very great quantity of salt. There are likewise salt springs at Wologda, Novgorod, Archangel, and Olonetz; and in the governments of Tomsk and Irkoutsk, in Siberia. The sale of salt is in the hands of Government, and they lose a great deal of money by the trade. The Russian peasants are but ill supplied with salt, and do not consume so much as they would be inclined to do if they had it in their power.

31. Observations and Reflections on the Tides in the Harbour of Nangasaky in 1805. By Captain de Krusenstern. P. 530. Nangasaky is a harbour in Japan. The observations seem to have been made with great care. The general results were as follows:—The highest tides were the third or fourth after the syzygies; the least tides were the third or fourth after the quadratures. The retardation of the tides at the syzygies was 37′ 19″ at the quadratures 1h 6′

50″. The time of high water at the syzygies at Nangasaky is 7^h 52′ 41″.

Vol. III.

1. Method of integrating the Differential Equation $d y + y y d x = \frac{A d x}{(a + 2 b x + c x x)^2}$. By L. Euler. P. 3.

2. Concerning a remarkable Paradox that occurs in the analysis of Maxima and Minima. By L. Euler. P. 16.

3. Of the Summation of Series that come under this Form: $\frac{1}{a} + \frac{a^2}{4} + \frac{a^3}{9} + \frac{a^4}{16} + \frac{a^5}{25} + \frac{a^6}{36} +$, &c. By L. Euler. P. 26.

4. Of the Transformation of Functions involving two Variables when two other Variables are introduced in their Place. By L. Euler. P. 43.

5. Solution of a curious Question in the Doctrine of Combinations. By L. Euler. P. 57.

6. Of the Division of a Rhomboid into four equal Parts, by two straight Lines cutting each other at right Angles. By Nicolas Fuss. P. 65.

7. Illustration of the Method of integrating the Differential Equation $y d y + P y d x + Q d x = o$, P and Q being functions of x. By Nicolas Fuss. P. 75.

8. Solution of some Problems relative to the developement of Curve Lines of Double Curvature. By Nicolas Fuss. P. 91.

9. Some Trigonometrical Series deduced from the inverse Theorem of Taylor. By M. Pfaff. P. 108.

10. Of the mutual Relation between some Integrals. By C. F. Kausler. P. 114.

11. Summation of innumerable Series derived from the principles of the Integral Calculus. By C. F. Kausler. P. 137.

12. Observations made at the Observatory of the Academy. By F. T. Schubert. P. 152.

13. Continuation of the Dissertation on the Pollen of the Antheræ of Plants. By I. T. Koelreuter. P. 159. This is a minute description of the figure of the pollen of a great number of plants, at least 300 species. It is scarcely susceptible of abridgment.

14. Description of the Lilies of Japan. By C. P. Thunberg. P. 200. In this paper we have a description, with figures, of eight species of lily; namely, lilium pomponicum, lilium lancifolium, lilium elegans, lilium longiflorum, lilium maculatum, lilium japanicum, lilium speciosum, lilium cordifolium.

15. On the aluminous Stones of Mount Ararat. By B. Severguine. P. 209. Five varieties of *alum stone* are described, quite different in appearance from the alum stone of Tolfa. The first variety has the aspect of jasper, the second of opal, the third of breccia.

16. Remarks on the Cranium of the Musk-bison. By N. Ozeretskovsky. P. 215. This animal no longer exists in Siberia; but

skeletons of it are frequently found. A description of the cranium was formerly published by Pallas; but his specimen wanted the horns, and was imperfect in several other respects. The present paper gives the dimensions and a figure of a very perfect cranium of this animal, lately found and sent to the Academy by Count N. P. Roumiantzow.

17. On the Ganglion of the middle Branch of the descending Hypoglossal Nerve. By P. Zagorsky. P. 219. This is an account of an uncommon distribution of this Nerve, which is of very rare occurrence.

18. Description and Figures of the Fishes of Camtschatca. By M. Tilesius. P. 225. The fishes described in this paper are the following:—Gasterosteus cataphractus, ophidium ocellatum, petromyzon marinus Camtschaticus, pleuronectes stellatus, cottus hemilepidotus, myoxocephalus stelleri, synanceja cervus. Copious descriptions of these animals are given, and many curious facts are stated respecting their natural history and economy, according to the usual custom of Tilesius in all his papers.

19. Description of a Tetras, or of a Species of Bird very little known, which is to be found in the Neighbourhood of Petersburgh. By G. F. Langsdorff. P. 236. This bird had been supposed a hybrid animal produced from the tetrao urogallus (cock of the wood) and the tetrao tetrix (black grous); but M. Langsdorff has ascertained that it is a peculiar species, to which he has given the name of tetrao intermedius. He gives a description and figure of the bird. It is said to exist in Scotland, and likewise in Sweden.

20. Description of the Alyssum Rostratum and Erodium Serotinum. By C. Steven. These two plants were found by the author in the southern provinces of Russia. His descriptions are accompanied by figures.

21. Arrangement and Description of the Mammalia of the Cape of Good Hope. By C. P. Thunberg. P. 299. The author gives a list and description of 59 species of animals. The number of species belonging to the different genera are as follows:— Simia, two; myrmecophaga, one; canis, two; hyæna, one; felis, six; viverra, seven; meles, one; talpa, one; hyrax, one; arctomys, three; lepus, two; dipus, one; sciurus, one; hystrix, one; camelopardalis, one; antilope, 15; ovis, one; bos, three; equus, two; sus, two; elephus, one; rhinoceros, one; hippopotamus, one; phoca, two.

22. Meteorological Observations made at St. Petersburgh in 1803 and 1804. By the late Mr. Inochodzow. Drawn up by Basil Petrow. P. 304.

23. Of Things susceptible of having Value. Analysis of the different kinds of Goods. By Henry Storch. P. 349.

24. Analysis of the Notions of Individual and National Riches. By Henry Storch. P. 364.

25. Analysis of the Notions of Individual and National Capital. By Henry Storch. P. 382.

26. On the Number of Inhabitants in Russia, and the Progress of its Population, from Facts ascertained by order of Government.

By C. T. Hermann. P. 391. A translation of this interesting paper will be found in the present and the preceding Number of the *Annals of Philosophy*.

27. On the Distribution of the Population of Russia. By C. T. Hermann. P. 437. This paper contains so much valuable information that I consider it as highly worthy the perusal of the English reader. I shall therefore insert a translation of it either in the next Number of the *Annals of Philosophy*, or into the first succeeding Number in which space for it can be afforded. These two papers will put it in the power of the reader to draw pretty exact conclusions respecting the internal state of Russia, and the rank which she is capable of holding in the great European Republic.

ARTICLE XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday the 24th of February, a paper was read from Dr. Herchell, consisting of an arranged set of observations to enable astronomers to judge of the probability of his opinion respecting the origin of stars. It is well known to most of our readers, that Dr. Herschell has discovered and described a prodigious number of nebulosities in the heavens, and that he has been induced to conclude from his observations, that these nebulosities gradually collect together, and in that way form stars. The object of the present paper is still further to elucidate and confirm this opinion. Sometimes nebulosities appear all of equal brightness, exhibiting a milky whiteness every where alike, sometimes they are brightest towards the centre, sometimes a luminous spot appears in the centre, and sometimes there is a distinct star. The Doctor conceives that these are the gradual steps of the star-making process. In like manner two stars are frequently seen with a nebulous matter between them. But it would be difficult to give a connected view of the numerous observations, which were not very intimately connected together; though they exhibit all that ingenuity and all that originality of thinking for which Dr. Herchell is so conspicuous. He showed that the light of the stars differs as much from each other as that of the planets, and he conceives the stars to be opaque globes surrounded with luminous atmospheres like the sun; and sees no reason why they may not be inhabited. One set of his observations seemed to me rather hostile to his hypothesis. He showed that many of the nebulæ, when examined by very powerful telescopes, were found to be clusters of stars: hence a probable inference seems to be, that if our telescopes were sufficiently powerful, we should discover the whole of the nebulæ to be in the same predicament.

On Thursday the third of March the remainder of Dr. Herchell's paper was read. Several nebulosities seem to have surrounded certain stars in consequence of a motion which they had acquired, and which brought them within the stars' spheres of action. Dr. H. likewise noticed clusters of stars which seem mutually to attract each other, as they are densest in the centre. These clusters are chiefly in the Milky Way.

On Thursday the 10th of March a paper by Mr. Sepping was read, on an improvement in the mode of building ships of war. Notwithstanding the importance of our navy to Great Britain, and the increasing scarcity and price of oak, no improvement has taken place in the construction of ships of war for the last century. Mr. Sepping in this paper described an improvement which he himself has made, which adds to the strength and durability of ships, while in consequence of the advantage which it affords of using the oak of old ships, reduces the quantity of new oak necessary for a ship of war about $\frac{1}{7}$th, and saves about 140 oak trees in the building of a single 74 gun ship. According to the old mode of building, the different timbers were made to act on each other at right angles. According to the new they act obliquely. But it would be scarcely possible to convey an idea of the new method without drawings; nor indeed does the editor consider himself as sufficiently acquainted with the subject to venture upon details. Several ships have already been constructed according to the new plan; so that its comparative advantages will be put to a fair trial.

On Thursday the 17th of March a paper by Dr. Chrichton of St. Petersburgh was read, on the means by which vitality is supplied to the living system.

Dr. Crichton conceives that there is a continual waste of vitality during life, and therefore that a regular supply is necessary. He thinks that this vitality is furnished by the food, and believes that the food contains particles endowed with vitality, and that this vitality is neither destroyed by the destruction of the organic texture, nor by the heat to which the food is exposed. He made decoctions of camomile, feverfew, nutgalls,&c. in distilled water, put the decoctions into glass jars inverted over distilled mercury, and introduced into them oxygen gas obtained from black oxide of manganese. Numerous confervas made their appearance in these decoctions, and considerable portions of the gas were absorbed. From these experiments he draws as a conclusion, that there are two kinds of particles of matter, namely organic particles and inorganic particles, and that the vitality of the first is not destroyed by boiling water. In general he found that vegetation commenced soonest when the decoction of flowers is used, and latest when that of roots. These experiments lead directly to the doctrine of equivocal generation, and prove nothing, unless that doctrine be taken for granted. Similar experiments were long ago advanced by Girtanner in support of equivocal generation, and he modestly boasted that he had *created* a vegetable. I can conceive the seeds of the confervas in question to have existed in the distilled water, and to have risen with that liquid in the state of vapour. The water, to do away such an objection, ought to have been passed through a red hot tube in the state of vapour. In short, the experiments are far from decisive, and it would be a very difficult task to execute decisive experiments on such a subject.

LINNÆAN SOCIETY.

On Tuesday the 1st of March a biographical account of Mr. James Don, curator of the botanical garden at Cambridge was read. He appears to have been a well informed and industrious man; though his literary labours were confined to the drawing up of a catalogue.

On Tuesday the 15th of March was read a paper by Dr. Smith, the president, proving the lepidium nudicaule of Linnæus, to be a species of the new genus tesdalea, lately established by Mr. Brown. Mr. Brown had referred to it only one species, the iberis nudicaulis of Linnæus, which is a British plant. Dr. Smith considers the lepidium nudicaule as a distinct plant, though resembling the other very closely. It grows at Montpelier and in the south of France.

At the same meeting there was read a description of a new species of warbler, by Dr. Trail of Liverpool. He got the skin of the bird from Brazil, and he considers it as a new species, to which he gives the name of motacilla xanthopa. It is chiefly distinguished by two yellow spots behind the eyes.

Dr. Trail terminated his paper with some observations on the bill of the toucan, which is well known to be of a monstrous size when compared with that of the bird. It was considered as hollow; but Dr. Trail has shown that it contains within it a boney matter with a fine tissue of blood vessels communicating with the nasal organs of the bird. He conceives it intended to give the animal a very perfect sense of smell, in order to enable it to pick out its food in the almost impenetrable forests where it is destined to live.

WERNERIAN SOCIETY.

At the meeting on the 21st of January, Professor Jameson read the first part of a mineralogical description of the county of Fife. In this communication, he confined his observations and remarks to the country around Burntisland. The whole of this small but curious tract of country is composed of floetz and alluvial strata, and affords an admirable study for the mineralogist. Although the strata, upon the whole, are well exposed, yet their structure, extent, magnitude, position, and alternation, are not to be ascertained by a rapid examination or cursory view, but will occupy even the experienced naturalist for weeks. The floetz rocks are sandstone, lime-stone, slate-clay, bituminous shale, clay-ironstone, basalt, greenstone, wacke, amygdaloid, and trap-tuff. The lower and middle parts of the district are composed of an alternation of greenstone, sandstone, limestone, slate-clay, &c.: the upper part is composed of trap-tuff, wacke, amygdaloid, and basalt. The sandstone rocks contain vegetable impressions and coal; and show a transition from pure quartz to sandstone;—a fact which, in connexion with others stated by Mr. Jameson, illustrates the chemical nature of sandstone. The slate-clay presents a curious connexion with felspar,—an appearance in favour of the chemical nature of slate-clay, and of the connexion of slate-clay as a member of the felspar series. In the limestone strata are situated the well-known lime quarries of Dalgetty. The trap rocks contain veins of trap; also of sandstone, limestone, and slate-clay, and portions of slate-clay and limestone resembling fragments: all of which appearances Professor Jameson considers as chemical contemporaneous formations; and he concluded by remarking that probably the prevalent theory of the mechanical formation of floetz rocks would be found to be less consonant to nature than the hypothesis of their chemical formation now proposed.

At the meeting 12th February, Dr. Macknight read a paper on the Cartlane Craig: a vast chasm in sandstone rocks above Lanark, formed by the lower part or projecting shoulder of a great mountain-mass, detached from the body or upper part, and extending more than three quarters of a mile in a curved line from S. W. to N. E., with a depth of several hundred feet. To ascertain how this enormous and striking fissure has been produced is a curious geological problem; the more interesting, that the phenomena of the Cartlane Craig are such as to furnish a remarkable test for trying the merits of the two theories which now divide the geological world. According to the principles of the igneous theory, a vein of trap, which traverses the strata in a direction almost perpendicular to the course of the chasm near its centre, renders it an example on a great scale of disruption and dislocation by explosion from below. On the other hand, the Cartlane Craig evidently possesses all the data requisite to form a case of what is called in the aqueous theory, *subsidence*: an explanation which Dr. Macknight is inclined to prefer, because the trap, from the smallness of its mass, seems totally inadequate, as a mechanical power, to the effect produced; because the direction of the rent, instead of following the course of the vein, which it must have done had it owed its existence to this cause, is very nearly at right angles to that course; and because it appears on examination that the trap itself had been originally a part of the formation or mountain mass, previous to the time when the rent took place.—The Cartlane sandstone belongs to the oldest of the floetz rocks. In the under part of this formation, it alternates with grey-wacke, and contains lime in calc-spar veins. Some varieties are good specimens of what Mr. Jameson considers as chemical depositions. The trap consists of compact greenstone; basalt including olivin and augit; and a substance intermediate between basalt and clinkstone.

At the same meeting, the secretary read a communication from Dr. Thomson, containing a description and analysis of a specimen of lead ore from India. It appeared to be a chemical combination of the sulphurets of lead, copper, and iron, in the following proportions:

Sulphuret of lead	57·269
Sulphuret of copper	40·850
Sulphuret of iron	2·190
	100·309

This ore, supposing the iron to be accidental, consists of one integrant particle of sulphuret of lead combined with two integrant particles of sulphuret of copper; and hence the Doctor was inclined to consider it as a new species of lead-ore, of little value however in a metallurgic point of view.

At this meeting, also, there was presented to the society a branch of mimosa decurrens, containing several bunches of flowers, the first time they have been produced in this country. The plant is in the fine conservatory at Milburn Tower, the seat of the Ambassador Liston; it is fifteen feet high, and has been thrown into a flowering state by the judicious management of Mr. Joseph Smaill, the gardener, who checked its growth, by cutting some of the roots, and by substituting a proportion of sand for rich earth.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute of France during the Year 1813.

(*Continued from* p. 234.)

Memoir of M. Burckhardt on the Quantity of Matter in the Planets.

The formulas of M. M. Lagrange and Laplace enable us to assign for any epoch the situation and dimensions of the planetary orbits. Mathematically speaking, the problem is resolved. The arbitrary constant quantities are mostly determined with a sufficient degree of accuracy for these researches. We are acquainted even with the quantity of matter in those planets which have satellites, as is the case with Uranus, Saturn, Jupiter, and the Earth; but Mars, Venus, and Mercury have no satellites. We have no other means of determining the quantity of matter in them than the alterations which they produce in the eccentricities and inclinations, or the equations which they give for the movements of the aphelions and nodes. But these variations are extremely slow. Good observations go no further back than 60 years. There remain only the periodic equations of the longitude. These equations are not greater; but their periods are shorter. Half a period is sufficient to obtain a double effect, since it is alternately positive and negative. The moon is almost in the same situation with Venus. Notwithstanding the assistance drawn from the tides and from the nutation, we have not yet an exact knowledge of the quantity of matter in our satellite.

Yet unless we adopt at least a hypothetic value for these unknown quantities, it is impossible to have exact tables of the apparent motion of the sun. Fortunately during the last sixty years we have a prodigious number of good observations. The values of the quantity of matter in these planets, which agree best with the totality of these observations, will be, if not certain, at least the most probable values of these doubtful quantities.

In order to determine them, the author of the tables of the sun had chosen, out of all the observations which he had calculated, all those where each of the quantities of matter in the planets produced sensible effects. The results at which he arrived did not appear even in his own eyes so certain, that they might not be somewhat changed either from other observations or from the same observations differently combined, especially if different elements be used in reducing them, such as the right ascensions of the stars.

It is the same with the mean secular motion of the sun. He had determined it by the comparison of a great number of observations made about 1752 and 1800, which only gave him the movement of 48 years, that is a little less than one half of the secular motion. He presented this movement not as certain, but as agreeing best with the observations that he had calculated. He perceived, himself, that the slightest change in the position of the stars at the two extréme epochs would introduce an equal change in the movement obtained. He did not venture to affirm that this movement was preferable to that which M. de Zach proposed about the same time; and which was the same with that upon which he himself had fixed in his first researches. He remarked in his preface that *time alone would decide upon a point so delicate.*

But what will certainly be obtained in a long interval of time, and by more numerous and more precise observations, may likewise be obtained at least in a certain degree by redoubling the care, multiplying the calculations, and employing more correct data. This is what M. Burkhardt has attempted, and he has neglected nothing to obtain a successful result.

He began by calculating 36 years of observations from 1774 to 1810: in order to be able to neglect without inconvenience the small equation of the latitude of the sun, which ought to pass several times over all the values of which it is susceptible, in this double revolution of the nodes of the moon. He employed besides 310 observations of Bradley in 1752. By this means he gained already 8 years, which have elapsed since the construction of the last tables. To calculate these observations he took a mean between the corrected right ascensions of Maskelyne and those of M. Bessel. The author of the tables had employed the right ascensions of Maskelyne corrected by his own observations in 1800. And for 1752 he had taken the right ascensions of Bradley, newly corrected by Hornsby the editor of Bradley. From these changes, which the researches and observations made of late years rendered possible, there ought to result a difference in the mean motion, and probably likewise in the value of the quantity of matter in the planets.

M. Burckhardt finds 3·8″ to be added to the motion for 49 years, which gives 7·7″ for the secular motion. This motion is the mean between that of Zach and that which La Caille had published about 56 years ago. Mayer, who had attempted to correct this motion, had considerably increased the small error in it. Lalande had diminished Mayer's motion 20″; and had given a quantity 8″ greater than that now given by Burckhardt.

We see at least that in these oscillations the error is always diminishing, and that if we have not yet obtained the true quantity, we have made a sensible approach to it. The mean length of the year, according to M. Burckhardt, is 365 days, 5 hours, 48′ 49·7″. The author of the tables made it 51·5″. But in the second volume of his astronomy, which was published about a year ago, we shall find that he inclines to 50″, and was therefore himself approaching to the new determination. The difference is now only 5·3″ a quantity respecting which it will be always difficult to decide.

The author of the tables had found for the lunar equation 7·5″; La Caille supposed 7.05″, Maskelyne 7·1″, M. Burckhardt 6·8″. The mean will be 0·15″. So that the uncertainty is reduced to a small fraction of a second.

M. Burkhardt finds that the quantity of matter in Mars previously determined must be diminished $\frac{1}{20}$. Now as the equation itself is very small, this correction, very uncertain on many accounts, does not deserve any attention.

The most considerable correction is that for Venus; and as the equation is much more sensible, the uncertainty ought to be much less. M. Burkhardt diminishes its quantity of matter $\frac{1}{9}$, which will produce a diminution of about 1″ in the greatest equation.

The quantity of matter was supposed by Laplace	1·0000
The author of the tables made it	1·0743
M. Burkhardt reduces it to	0·9549
M. Lindenau has lately found it	1·0797
and	1·1131

The mean of these four results is 1·0555, and differs only $\frac{1}{5[illegible]}$ from that supposed in the tables.

It was by the movements of Mercury that M. Lindenau endeavoured to determine the quantity of matter in Venus. He has united the results which he obtained from the motions of the aphelion and the nodes. His mean is 1·0964, so that he augments the equation of the tables as much as we should diminish them, if we were to make choice of the above mean. Between these opposite testimonies, the author of the tables may adhere to his own number. But he puts no greater confidence in his determination than in any other. He will even agree that the result obtained by M.

Burkhardt, founded upon a greater number of observations, and upon newer researches, offers in consequence a greater degree of probability. A reason of great weight adds strength to the necessity of diminishing the mass of matter of Venus, and this reason long excited distrust in the author of the tables. In whatever manner he combined the observations of Lacaille, Mayer, Bradley, Le Gentil, Maskelyne, Piazzi, and his own, he could never obtain more than 48″ for the secular variation of the obliquity of the ecliptic. The mean obliquity which he found in 1800 has been since confirmed by all the solstices observed at Paris. That which results for 1750, from so many observations agreeing remarkably well with each other, cannot be wrong further than 1″. Hence he concludes, that the secular diminution cannot be greater than 50″. He has never believed that it amounted to 52″. We may therefore suppose to Venus a quantity of matter which would give 48″ or 50″ for the secular diminution, and give to the equation of Venus in the solar tables, the value which will result from this supposition.

M. Burkhardt proposes a diminution of 1″ for the greatest equation of the centre. If we collect together all these corrections, it will follow, that the sun's place, calculated at present for 1850, may differ 6″ from a perfect observation made at that time. But before this can happen we must suppose what is scarcely possible, that the three errors are all a maximum, and have the same sign. The greatest error that can be supposed is 3″ at the end of 50 years. We wish the instruments by that time may be brought to such perfection that an astronomer can make a single observation with no greater an error. But this slight error may be easily avoided by adopting the corrections of M. Burckhardt. The most important of them is that of the secular motion. It is easy to diminish it $\frac{1}{10}$ of a second per year. The two other corrections would give still less trouble; but as they are periodic, and often almost nothing, they may in many cases be neglected.

Corrections, so little sensible as not to pass the limits of the errors of the best observations, may pass for a confirmation of the tables, as well as for an amelioration of them. We may be even sorry for astronomers devoting themselves to calculations so long and so fastidious, and yet obtaining only results so little different from those which we possessed before. But the tables of the sun constitute the foundation of all our calculations: they cannot be too frequently verified. It is particularly the duty of the members of the Board of longitude to attend to this verification. It was on this account that M. Burckhardt has undertaken a still more laborious investigation of the tables of the moon, in order to obtain ameliorations of the same kind. The very smallness of these corrections is a most satisfactory proof of the singular perfection which astronomical observations and calculations have reached.

(*To be continued.*)

Article XIII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Mr. Singer will commence his Lectures on Natural Philosophy on Monday, the 4th of April. A card of the arrangement may be had at the lecture-room, 3. Princes-street, Cavendish-square; or of Mr. Triphook, 37, St. James's-street.

II. *On New Properties of Light exhibited in the Optical Phenomena of Mother-of-Pearl.**

The splendid exhibition of colours which distinguishes mother-of-pearl from every other body, and the successive developement of fresh tints by every gentle inclination of the plate, do not arise, as has always been supposed, from the lamellated structure of the shell, but are owing to a new power of *extraordinary reflection*, and to a new faculty of separating the light into its component parts. Both of these powers demonstrably *reside without the surface of the mother-of-pearl*, and the light which produces the brilliant colours *has never penetrated the substance of the mother-of-pearl*.

When we look at the image of a candle reflected from a piece of regularly formed mother-of-pearl *ground upon a hone, but not polished*, we perceive on one side the common image, and at the distance of 4 or 5°, an extraordinarily reflected image highly affected with the prismatic colours. The distance of this image from the common image, or the angle of aberration, increases with the angle of incidence, and the lines of the angles of aberration are inversely as the lines of the angles of extraordinary reflection. By *polishing* the mother-of-pearl *a new image, exactly like the first, and obedient to the same laws, is developed on the other side of the common image.* Two coloured images are also seen by transmission, and the image which is faintest by reflection is always brightest by transmission.

The most remarkable circumstance, however, is, that the optical properties, which have now been described, *can be communicated by pressure to wax, cements, gum arabic, balsam of Tolu, realgar, tin foil, the fusible metal made of bismuth and mercury*, and even to *lead* by hard pressure, or by the blow of a hammer. All these substances shine with the same brilliant colours as the mother-of-pearl itself, and prove beyond all question that the colours are produced by some particular configuration of surface, which is communicable to soft and fusible substances, and which cannot be affected or removed by the finest polishing.

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* We are favoured with this and the following notice by Dr. Brewster.

By applying high magnifying powers, I discovered that mother-of-pearl had an elementary grooved structure like the delicate texture of the skin of an infant's fingers, and that the *direction of the grooves is always perpendicular to the axis of extraordinary reflection.* In some pieces of mother-of-pearl these grooves can be seen with a power of 8 or 10 times. In other pieces it requires a power of 400 to see them: and in some pieces they cannot be detected by any power which I have been able to apply. In irregularly formed mother-of-pearl the grooves are arranged circularly, like the veins in the hammered agate, and exhibit a number of very curious appearances. The same grooves are seen on the surface of the wax and metals after they have been impressed with the mother-of-pearl. These experiments lead to a number of important optical conclusions, which could scarcely be understood in a brief notice like the present; and they prove incontrovertibly that there exist near the surface of bodies new forces which act upon light, and which are totally different from the ordinary forces which produce refraction and reflection.

III. *On a New Method of Polarizing Light peculiar to Mother-of-Pearl.*

This substance possesses the singular property of polarizing light in a way different from all bodies, whether crystallized or uncrystallized. In all doubly-refracting crystals the opposite polarization of the two images is always related to some axis or fixed line in the primitive form; while in all uncrystallized bodies, such as a bundle of plates of glass, or a bundle of films of gold-beater's skin, the polarisation is related to the planes of reflection and refraction, the reflected pencil being always polarised in an OPPOSITE manner to the refracted pencil, like the two images formed by calcareous spar. In mother-of-pearl, however, a *single plate possesses* the property of *polarizing the whole of the transmitted light at an angle of incidence of* 60°; and what is still more extraordinary, the *transmitted pencil is polarized in the* SAME *manner as the reflected pencil.* By turning the plate of mother-of-pearl about its centre so as to preserve the same inclination to the incident ray, no change whatever takes place. Mother-of-pearl also polarizes light, and exhibits the coloured rings which are described in the *Annals of Philosophy*, N° XV. p. 193.

IV. *Salt sublimed during the burning of Bricks.*

I received some weeks ago from Mr. Trimmer a specimen of a salt which, he informed me, made its appearance on the top of brick-kilns during the burning of bricks. This salt, I find, has the following properties:—1. It is in powder or in small lumps, and has a white colour. 2. Its taste is saline and cooling, precisely the same with the taste of sal ammoniac. 3. When heated it does not melt, but sublimes completely in a white smoke, which has the well-known smell of sal ammoniac, and may be condensed by presenting to it a piece of cold iron. 4. It effervesces in sulphuric acid, and gives out abundance of muriatic acid. 5. When thrown into potash ley, it gives out a strong smell of ammonia; and when the mixture is heated, a strong effervescence takes place, and ammoniacal fumes are given off in abundance. These properties leave no doubt that the salt is *sal ammoniac*. It is nearly pure; for I find that it sublimes without leaving any other residue than a slight trace of earthy matter.

It is not easy to explain the formation of this salt. The coals used as fuel would supply the ammonia; but I do not see any source from which the muriatic acid can come. Perhaps Mr. Trimmer himself may be able to favour us with an explanation, from his knowledge of the substances mixed with the clay before it is made into bricks. The subject is curious, and deserves investigation.

V. *Precession of the Equinoxes.*[*]

The Royal Academy of Sciences in Berlin had proposed, as a prize, the most accurate determination of the magnitude of the precession of the equinoxes. The prize has been adjudged to Mr. Bessel. No person could have performed this task with more fortunate success than Mr. Bessel, who has been employed for six years in an examination of the Bradleian observations. During these researches he made use of no fewer than 4585 observations of the stars. We conceive we shall perform an acceptable service to astronomers if we state the result he obtained of one magnitude, which is daily used:—

Luni solar precession $= 50{\cdot}35330'' - 0{\cdot}0002435890'' \, (t - 1800)$:

General precession observed: $50{\cdot}18924 + 0{\cdot}0002442966'' \, (t - 1800)$:

Constant quantity by the precession in right ascension: $46{\cdot}01058'' + 0{\cdot}0003590677'' \, (t - 1800)$:

Constant quantity by the precession in declination: $20{\cdot}04966'' - 0{\cdot}0002135621'' \, (t - 1800)$:[†]

VI. *Method of ascertaining the Presence of Manganese.*

I have been requested by a Correspondent, who subscribes himself E., to inform him of a good test for manganese. He mentions some unsuccessful results of his own to attain his object. I may observe, in answer, that the colour of metallic precipitants by prussiate of potash, nutgalls, and hydrosulphurets, must always be ambiguous when various metals happen to be in solution together, unless the experimenter possesses much practical knowledge. I shall make a few observations, which may perhaps be of service to my correspondent.

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[*] This notice is translated from the Gottingische gelehrte Anzeigen for January, 1814.

[†] It is scarcely necessary to observe that *t* in these formulas signifies the year of the Christian era. The results obtained by the French astronomers respecting these very quantities will be seen in the account of the Institute in the present Number of the *Annals of Philosophy*.

1. Suppose a piece of matter is presented to us, and we wish to know whether it contain a notable quantity of black oxide of manganese. 1. Reduce it to powder, and pour upon it muriatic acid; then apply a moderate heat. If chlorine be disengaged in abundance, your ore is chiefly manganese. 2 Melt a little borax or soda upon a thin plate of platinum by the blow-pipe, add a little of your ore, and keep it melted by the interior flame of the candle. The colour will be at first red, but will gradually disappear if the ore be manganese. Now add a little nitre, or keep it melted for some time in the exterior flame, and the red colour will again appear.

2. The method of separating manganese from iron has been explained in a preceding Number of the *Annals of Philosophy*. The solution of pure manganese in acids is colourless; it is precipitated by alkalies white, but becomes speedily black when left upon a filter. This is the usual test of manganese employed by chemists, and it is quite sufficient to distinguish this metal from all others. Manganese is not precipitated from acids by bicarbonate of potash.

VII. *On the Degree of Cold obtained by Professor Braun.*

An anonymous Correspondent from Ireland has put to me the following question:—" In the Translation of Haüy's Natural Philosophy by Dr. Olinthus Gregory, vol. i. p. 200, he says, (speaking of Mr. Braun,) the mercury continued to descend, and arrived in the last experiment at — 352° of Fahrenheit. Now I am unwilling to believe that the intensity of cold mentioned by Haüy has been obtained. Your opinion on the subject I should consider as a great favour."

Haüy's book was a hasty and imperfect performance, suddenly written, and published by order of Bonaparte. Hence we need not be surprized at the numerous omissions so perceptible in it. Braun, in his original account of his experiments, states the cold to have been as intense as — 352°. He was deceived by this circumstance. When mercury freezes it contracts about $\frac{1}{20}$th part of its volume. Braun supposed this contraction the consequence of cold, whereas it was the consequence of the congelation of the mercury. We cannot determine any degree of cold below — 40° by a mercurial thermometer. Therefore the cold obtained by Braun remains unknown. His mistake was first pointed out and explained by Mr. Cavendish.

VIII. *Iodine.*

In answer to the observations and inquiries of the same gentleman respecting the preparation of iodine, I may say that the yellow precipitate is usually a compound of iodine and sulphur, and constitutes the objection to the mode of obtaining iodine in the way I formerly mentioned. I shall therefore take the liberty of giving here Dr. Wollaston's method of obtaining it, which I have likewise tried, and find more productive than my own.

Dissolve the soluble part of kelp in water. Concentrate the liquid by evaporation, and separate all the crystals that can be obtained. Pour the remaining liquid into a clean vessel, and mix with it an excess of sulphuric acid. Boil this liquid for some time. Sulphur is precipitated, and muriatic acid driven off. Decant off the clear liquid, and strain it through wool. Put it into a small flask, and mix it with as much black oxide of manganese as you used before of sulphuric acid. Apply to the top of the flask a glass tube shut at one end. Then heat the mixture in the flask. The iodine sublimes into the glass tube. Dr. Wollaston informs me that soapers' black ashes yield iodine in considerable quantity. Mr. Tennant tried sea water without finding any in it; so that it would appear to be derived from the sea weed entirely.

IX. *Basaltic Rock near Nottingham.*

I have been favoured by the Rev. Mr. Toplis with specimens of a basaltic rock which he found near some sand rock in a valley by the side of the foot-path leading from Clifton to Barton, near Nottingham. The specimens which I received constitute a black porous rock, with white particles in it. The appearance is strikingly similar to that of Mr. Gregory Watts's fused greenstone; and I cannot avoid thinking it has undergone the action of heat. I cannot gather from Mr. Toplis's information whether it was a rock in situ, or a loose rock, from which he detached the fragments. The white specks may be felspar; but the mineral characters of the fragments which I received are rendered so indistinct, either by heat or the weather, that it would not be easy to determine the species to which they belong. In one of the stones there is a round nodule of quartz. As Mr. Toplis's letter contains some valuable facts respecting the mineralogy of the vicinity of Nottingham, I shall insert the greatest part of it here:—

"As I never heard of any basaltic rocks having been found in this neighbourhood, I thought it deserving of notice: it is vesicular, and has white quartz pebbles interspersed throughout it.

"The only rock hitherto observed in the vicinity of Nottingham is a white sandstone, containing also beautiful white quartz pebbles: it seems in some respects to be similar to the second sandstone formation described by Professor Jameson.

"Where this sandstone rock is covered with clay, considerable quantities of gypsum, principally fibrous, occur; as is the case on Snenton Hill, within half a mile of Nottingham. There seems, indeed, to be immense quantities of gypsum in this part of the county: it is in very large beds in the neighbourhood of Gotham and the adjacent villages, and it appears to extend itself from Derbyshire quite through Nottinghamshire, great quantities being found within a mile of Newark. I observed it about 300 yards from the place where the enclosed specimens were broken off."

X. *Caoutchouc Catheters.*

I insert the following communication just as I received it, regretting that I cannot communicate any information on the subject, but perhaps some of my readers may:—

"A Correspondent of yours is desirous of knowing how the elastic gum catheters are made. This is not generally known; for the manufactory of these very important instruments is, I believe, confined to one; a Mr. Walsh, formerly of Catherine-street, Strand, now of Chelsea; but, whether owing to a more profitable pursuit, his supply his irregular, and his conduct not the most accommodating. When they are made, they are very inferior to those made at Paris. Whether this depends on the original structure, or from age, I am unable to say.

"The first person who made them in this country was, I believe, a Mr. Whyat, surgeon, in the Strand, who is now dead; and with him died the secret, unless some of that ingenious family possess it, and consider the object unworthy their cultivating.

"There is woven on a metal rod extremely fine soft silk, of divers sizes, leaving an aperture, or, as the Parisians', two openings. These rods so covered are dipped in a solution of the elastic gum, and rubbed with a polished stone: then again dipped, and rubbed till they are of sufficient thickness and firmness. This is, I am told, a tedious process, and demands an uninterrupted attention until completed.

"It is generally considered that pure ether is that which is used for dissolving the *caoutchouc*.

"The irregularity with which the Profession has been supplied has induced several to obtain supplies occasionally from Paris; and they who have compared them with those made in London, under a continued use in the bladder, will readily admit my statement. I regret that the charge is too well founded. We ought not to be so outdone in an article of such usefulness. A SURGEON."

XI. *Method of preserving Vaccine Matter.*

Dr. Reid Clanny, of Sunderland, has favoured me with the following communication:—

"Permit me to detail to you a most convenient and useful manner of taking and preserving vaccine or variolous virus, which the faculty of this town have found to be much superior to any other. It is the invention of a Mr. Forman, an ingenious glass-manufacturer upon the Wear, near Sunderland. It is in the form of a small glass ball with a tube issuing from it, very similar to a cracker, as it is called, which mischievous boys put into candles to cause an explosion. The pustule from which the virus is to be taken being punctured by a lancet in the usual manner, the small ball or bulb is to be heated at a candle so as to rarify the air within it, and after it is sufficiently warmed the end of the little tube is to be inserted where the lancet had made the puncture, and the virus will immediately be taken up, so as to fill the bulb. The end of the tube is now to be hermetically sealed by means of a common blow-pipe at the flame of the candle, which is a very simple process; and thus the virus may be preserved for any length of time, and sent to any distance. If for immediate use, the tube need not be sealed, but may be secured in any convenient manner. Any requisite number of these balls may be employed, and it is proper to remark that the virus is never heated much above blood heat. I need add nothing in praise of the invention: it speaks sufficiently for itself, and has been used here for several years."

XII. *Insect on Apple-Trees.*

I insert the following query at the request of a Correspondent:—

"There is an insect, a species of mealy glutinous creature, which eats the bark of apple-trees so much that it destroys the tree. Washing with lime does not eradicate it. What else might be substituted effectually to kill it?"

XIII. *Mineralogy in Spain.*

The following extract of a letter from a scientific friend, who is well acquainted with mineralogy and geognosy, will, I am persuaded, gratify every mineralogical reader:—

"I have gleaned very considerable information in the course of my travels through Spain, although the present state of the country is very unfavourable to scientific research. The interior of the peninsula is over-run by hordes of guerillas, who now rob and murder all that they encounter. A careful examination of the country is thus out of the question; but I have availed myself of every means within my reach; the results of which I hope to be enabled to transmit to you, and my other scientific friends, in the course of a few months. I feel very much the want of a portable barometer, as I have had some good opportunities of ascertaining several heights. The plains of Castile are highly elevated, I apprehend, exceeding considerably 1000 feet. I judge from the difference between their height and that of the Mountains of Santander, contrasted with the apparent elevation of the latter above the sea. The thermometer fell only about 5° on gaining the highest points of that range from the plains of Castile; but it rose upwards of 16° on descending to the sea shore on which the town of Santander stands. I shall certainly visit Cadiz, and examine the quicksilver mine, as well as that of Gadalcanal. I have not yet found either andalorezite or arragonite. Near Santander I found in a cavity of the common limestone a considerable imbedded mass of yellow amber. Although within sea water mark, it was so firmly connected with the rock that I am induced to believe that it has been exposed by the destruction of a part of the surrounding limestone. Slaty pitch coal occurs in thick seams near Reynosa, in Old Castile; at Gijon, in Galicia; and at Laredo, in the province of Burgos. I have also, both in Spain and Portugal, observed very generally clay slate hills capped with quartz distinctly stratified."

ARTICLE XIV.

New Patents.

JOHN SUTHERLAND, Liverpool, coppersmith; for an improvement in the construction of copper and iron sugar-pans and sugar-boilers, and a new method of hanging the same; and also an improvement in the construction of the furnaces or fire-places in which such pans and boilers ought to be placed. Dec. 20, 1813.

WILLIAM SPRATLEY, London, coal-merchant; for an improvement upon the axletree of wheels for carriages of different descriptions. Dec. 20, 1813.

WILLIAM ALLAMUS DAY, Poplar, Middlesex; for a method of extracting all the gross or mucilaginous matter from finks, or Greenland blubber, produced from whales when boiled into oil; which method not only renders the oil so boiled more free from its usual rancid smell and taste, but in a great degree adds to its burning and inflammable qualities. Dec. 20, 1813.

JAMES CAVANAH MURPHY, Cavendish-square, London, architect; for an Arabian method of preserving timber and various other substances from corruption or decay. Dec. 24, 1813.

RALPH SUTTON, Birmingham, brass-founder; for an effectual security to prevent the accidental discharge of fowling-pieces; which invention is unconnected with the lock, and applicable to all kind of fire-arms. Dec. 24, 1813.

SIR THOMAS COCHRANE, Knt. commonly called Lord Cochrane; for methods of regulating the atmospheric pressure in lamps, globes, and other transparent cases; of supplying combustible matter to flames, and preserving uniform intensity of light. Dec. 24, 1813.

WILLIAM STOCKER, of Maltock, Somersetshire, gunsmith; for a cock made of metal and wood for drawing liquor from casks, which produces a stop superior to that which is effected by common cocks, and prevents the liquor from coming in contact with the metals, except when it is in the act of being drawn and is running from the cask. Jan. 10, 1814.

JOHN DUFFY, jun. Ballsbridge, near Dublin, calico printer; for a method of producing patterns on cloths made of calico or linen, or both, by preserving or defending mordants or colours previously applied to them from injury, when it is required to pass such mordants or colours through solutions of acids, of acid salts, of metallic salts, or of combinations of the oxymuriatic acid. Feb. 8, 1814.

John Vallance, jun. Brighthelmstone; for an apparatus for certainly cooling brewers', vinegar makers', and distillers' worts, wash &c. Feb. 8, 1814.

Timothy Harris, Foley-place, Portland Chapel, London; for a machine or machines for ploughing or laying on colours called grounds, printing, flocking, and pressing, so as to produce an even smooth face upon paper, silk, linen, woollen, leather, cotton, and various other articles. Feb. 8, 1814.

John Kershaw, Glossop-dale, Derbyshire, cotton-spinner, and **John Wood**, of the same place, Gentleman; for a mode of preparing flax for the purpose of being spun on the like machinery as cotton. Feb. 10, 1814.

Joseph Bramah, Pimlico, Middlesex; for a mode of applying a certain species of earth, which will prove useful, and be found productive of great public benefit, in as much as it will when applied prevent, destroy, and finally extirpate what is called the dry or fungus rot, and will serve as a substitute for lead in making of oil paints, and also for various other useful purposes. Feb 10, 1814.

Richard Price, Bristol, ironmonger; for an improved cooking apparatus. Feb. 12, 1814.

William Francis Hamilton, Asylum-buildings, London, engineer; for certain improvements in optical instruments and apparatus. Feb. 12, 1814.

John Buddle, Wallsend, Northumberland, Gentleman; for a fire-pan or fire-lamps, in which small or inferior coals may be consumed in the place of large or round coals; and that he hath also found out and invented a fire-grate or fire-stove to be fixed at the bottom of the chimney in the ordinary mode, in which fire-grate or fire-stove small or inferior coals may be consumed on all occasions, and for all the same purposes as larger or round coals. Feb. 21, 1814.

ARTICLE XV.

Scienific Books in hand, or in the Press.

Dr. Benj. Heyne's work, entitled "Tracts Historical and Statistical on India," will be ready for publication early in April. The subjects are miscellaneous; but there is much information that will be interesting to the Chemist and the Geologist.

A new Volume of the Transactions of the Wernerian Natural History Society is on the eve of publication.

Dr. Badham has in the Press an Essay on the Diseases of the Chest which have their seat in the Mucus Membrane, Larynx, or Bronchiæ.

Mr. S. Banks will speedily publish a Treatise on Diseases of the Liver, and Disorders of the Digestive Functions; including admonitory Suggestions to Persons arriving from Warm Climates.

Article XVI.

METEOROLOGICAL TABLE.

		Barometer.			Thermometer.					
1814.	Wind.	Max.	Min.	Med.	Max.	Min.	Med.	Evap.	Snow &c.	
2d Mo.										
Feb. 12	S	29·94	29·92	29·930	48	39	43·5	—		☾
13	S E	29·91	29·87	29·890	46	37	41·5	—	·10	
14	N E	30·07	29·91	29·990	41	29	35·0	—		
15	N E	30·13	30·07	30·100	38	29	33·5	—		
16	N E	30·40	30·13	30·265	39	28	33·5	—		
17	N E	30·42	30·39	30·405	33	19	26·0	—		
18	N E	30·39	30·25	30·320	39	30	34·5	—	—	
19	N E	30·41	30·25	30·330	40	23	31·5	—		
20	Var.	30·41	30·35	30·380	31	18	24·5	—		●
21	S E	30·30	30·25	30·275	34	19	26·5	—		
22	E	30·30	30·22	30·260	32	21	26·5	—		
23	S E	30·22	30·15	30·185	32	18	25·0	—		
24	E	30·20	30·12	30·160	33	18	25·5	—		
25	S E	30·12	30·02	30·070	34	21	27·5	—		
26	N E	30·08	30·00	30·040	35	24	29·5	—		
27	S E	30·00	29·83	29·915	39	26	32·5	—		☽
28	S W	29·83	29·12	29·475	41	30	35·5	·25	3	
3d Mo.										
March 1	Var.	29·07	29·05	29·060	45	31	38·0	—	—	
2	S W	29·05	28·97	29·010	45	31	38·0	—	—	
3	E	29·28	29·05	29·165	42	30	36·0	—		
4	N E	29·59	29·28	29·435	35	31	33·0	—		
5	N E	29·88	29·59	29·735	34	28	31·0	—	—	
6	N E	29·88	29·77	29·825	34	28	31·0	—		○
7	E	29·85	29·77	29·810	32	21	26·5	—	—	
8	N E	29·85	29·76	29·805	33	26	29·5	—	—	
9	N E	29·76	29·66	29·710	34	27	30·5	—	—	
10	N W	29·66	29·58	29·620	35	29	32·0	—	—	
11	N E	29·70	29·45	29·575	41	32	36·5	—	—	
12	N E	29·99	29·70	29·845	39	21	30·0	—		
13	N	30·18	29·99	30·085	38	30	34·0	·28	·82	
		30·42	28·97	29·889	48	18	31·93	0·53	0·95	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation

REMARKS.

Second Month.—12. Cloudy. At half-past ten, a. m. after a peculiarly unpleasant atmosphere, with a breeze from S.E. during the morning, a shower of *burnt paper*, in fragments of various sizes, began to fall over the whole village. It continued so long, and descended from such a height, that we necessarily referred for its cause to some great fire in London. It proved to originate from the burning of the Custom House, (distant in a right line about five miles, S.) at which there happened an explosion of gunpowder. 13. Misty morning. 14. *Cumulus* clouds, beneath *Cirrocumulus* and *Cirrostratus.* 15. Fine morning: *Cumulus:* breeze. Evening twilight clear orange, and the stars brilliant. 16. Cloudy morning: strong breeze: fine clear day. 17. Fine day: *Cumulus* with *Cirrus.* 18. Much hoar frost: fine morning: some rain, evening. 19, 20. Hoar frost. 21. The same: the moon visible, and very well defined at six, p. m. 22, 23. Hoar frost. 24. The same: *Cirrus* clouds, in parallel stripes from N. to S. all day: min. temp. at the Laboratory, Stratford, 15°. 25, 26. Hoar frost. 27. Hoar frost: *Cirrus,* in stripes from N. to S.: lunar halo. 28. *Cirrostratus* appears: the stones grow moist, and the wind has a hollow sound.

Third Month.—1. Damp and cloudy: hollow wind: sleet, p.m. 2. Rain and sleet at intervals. 3—8. Snow at intervals: the country has become again white with snow. 9. Snow, more plentiful, in the night. 10, 11, 12. Snow at intervals: which, in northern exposures, lies to some depth.

RESULTS.

Prevailing winds Easterly.

Barometer: Greatest height ....30·42 inches;
Least ....28·97 inches;
Mean of the period ....29·889 inches;
Thermometer: Greatest height ....48°
Least ....18°
Mean of the period ....31·93°

Evaporation, 0·53 inch, the water in the gage being most of the time solid.

Rain and melted snow ....0·95 inches.

⁂ The barometer and thermometer for the first ten days of the third month were noted at the Laboratory, Stratford.

Some intended remarks on the *present* winter (as we are still obliged to term it) compared with a former one, are necessarily deferred.

Tottenham,
Third Month, 19, 1814.

L. HOWARD.

ANNALS

OF

PHILOSOPHY.

MAY, 1814.

ARTICLE I.

Biographical Account of M. le Comte Lagrange. By M. le Chevalier Delambre.[*]

JOSEPH-LOUIS LAGRANGE, one of the founders of the Academy of Turin, Director during 20 years of the Berlin Academy for the Physico-Mathematical Sciences, Foreign Associate of the Academy of Sciences of Paris, Member of the Imperial Institute and of the Board of Longitude, Senator and Comte of the Empire, Grand Officer of the Legion of Honour, Grand Cross of the Imperial Order of Re-union, was born at Turin, on the 25th of November, 1736. He was the son of Joseph-Louis Lagrange, Treasurer of War, and of Marie-Therese Gros, only daughter of a rich physician of Cambiano.

His great-grandfather, a captain of horse in the service of France, had gone over to that of Emanuel II. King of Sardinia, who established him at Turin by marrying him to a lady of Conti, of an illustrious Roman family. He was originally a Parisian, and a relation of Marie-Louise, dressing maid to the mother of Louis XIV., and afterwards wife of Francis Gaston de Bethune.

These details are of no importance for the illustrious mathematician, whose reputation renders his genealogy of no consequence to him; but they are not indifferent to France, who eagerly recalled him, and restored him to his ancient rights. His name, and that of his mother, show that he was of French extraction. All his works were written in French. The city in which he was born has become a part of the French empire. France therefore has indisputably the right of boasting of one of the greatest geniuses that have done honour to the sciences.

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[*] Translated from the Moniteur for the 17th, 18th, and 19th of January, 1814.

⁂ Our readers will remember that we presented them with a short biographical sketch of M. Lagrange at the commencement of the second volume of the *Annals of Philosophy*: we now, according to our promise, have the pleasure of laying before them a very masterly account of that eminent mathematician.

His father was rich, and had made an advantageous marriage; but was ruined by hazardous undertakings. Let us not, however, lament the situation of M. Lagrange. He himself viewed it as the first cause of all the good fortune that afterwards befell him. "Had I been in possession of a fortune," said he, "I should not probably have studied mathematics." In what other situation would he have found advantages that could enter into comparison with those of a tranquil and studious life, with that splendid series of discoveries in a branch of science considered as the most difficult, and with that personal respectability which was continually increasing to the very last period of his life.

His taste for mathematics did not appear at first. He was passionately devoted to Cicero and Virgil, before he could read Archimedes and Newton. He then became an enthusiastic admirer of the geometry of the ancients, which he preferred to the modern analysis. A memoir which the celebrated Halley had composed long before, to demonstrate the superiority of the analytic method, had the glory of converting him, and of teaching him his true destiny. He devoted himself to this new study with the same success that he had had in the synthesis, and which was so decided that at the age of 16 he was Professor of Mathematics in the Royal Military School. The extreme youth of a Professor is a great advantage to him when he has shown extraordinary abilities, and when his pupils are no longer children. All the pupils of M. Lagrange were older than himself, and were not the less attentive to his lectures on that account. He distinguished some of them, whom he made his friends.

From this association sprung the Academy of Turin, which in 1759 published a first volume entitled Acts of a private Society. We see there the young Lagrange directing the philosophical researches of the physician Cigna, and the labours of the Chevalier de Saluces. He furnished to Foncenex the analytical part of his memoirs, leaving to him the task of developing the reasoning upon which the formulas depended. In these memoirs, which do not bear his name, we observe that purely analytical method, which afterwards characterized his great productions. He had found a new theory of the lever. It constitutes the third part of a memoir, which was very successful. Foncenex, in recompense, was placed at the head of the Marine, which the king of Sardinia formed at that time. The two first parts have the same style and seem written by the same person. Do they likewise belong to Lagrange? He never expressly laid claim to them; but what may give us some light into the real author, is that Foncenex soon ceased to enrich the volumes of the new Academy, and that Montucla, ignorant of what Lagrange revealed to us during the latter part of his life, is astonished that Foncenex interrupted those researches which might have given him a great reputation.

M. Lagrange, while he abandoned to his friend insulated theorems, published at the same time, under his own name, theories which he promised to develope further. Thus after having given new formulas of the *maximum* and *minimum* in all cases, after having shown the insufficiency of the known methods, he announces that he will treat this subject, which he considered as important, in a work which he was preparing, in which would be deduced from the same principles all the mechanical properties of bodies whether solid or fluid. Thus at the age of 23 he had laid the foundation of the great works, which constitute the admiration of philosophers.

In the same volume he reduces under the differential calculus the theory of recurrent series and the doctrine of chances; which before that time had only been treated by indirect methods. He established them upon more natural and general principles.

Newton had undertaken to submit the motions of fluids to calculation. He had made researches on the propagation of sound; but his principles were insufficient and even faulty, and his suppositions inconsistent with each other. Lagrange demonstrates this. He founds his new researches on the known laws of dynamics, and by considering only in the air the particles which are in a straight line, he reduces the problem to that of vibrating cords, respecting which the greatest mathematicians differed in opinion. He shows that their calculations are insufficient to decide the question. He undertakes a general solution by an analysis equally new and interesting, which enables him to resolve at once an indefinite number of equations, and which embraces even discontinued functions. He establishes on more solid grounds the theory of the mixture of simple and regular vibrations of Daniel Bernoulli. He shows the limits within which this theory is exact, and beyond which it becomes faulty. Then he comes to the construction given by Euler, a construction true in itself, although its first author had arrived at it by calculations which were not quite rigorous. He answers the objections of D'Alembert. He demonstrates that whatever figure is given to the cord, the duration of the oscillations is always the same: a truth derived from experiment which D'Alembert considered as very difficult if not impossible to demonstrate. He passes to the propagation of sound, treats of simple and compound echos, of the mixture of sounds, of the possibility of their spreading in the same space without interfering with each other. He demonstrates rigorously the generation of harmonious sounds. Finally, he announces that his intention is to destroy the prejudices of those who still doubt whether the mathematics can ever throw a real light upon physics.

We have given this long account of that memoir, because it is the first by which M. Lagrange became known. If the analytical reasoning in it be of the most transcendent kind, the object at least has something sensible. He recalls names and facts which are well known to most people. What is surprising is that such a first essay should be the production of a young man, who took possession of a subject treated by Newton, Taylor, Bernoulli, D'Alembert, and Euler. He appears all at once in the midst of these great mathematicians as their equal, as a judge, who in order to put an end to a difficult dispute, points out how far each of them is in the right, and how far they have deceived themselves; determines the dispute between them, corrects their errors, and gives them the true solution, which they had perceived without knowing it to be so.

Euler saw the merit of the new method, and took it for the object of his profoundest meditations. D'Alembert did not yield the point in dispute. In his private letters, as well as in his printed memoirs, he proposed numerous objections, to which Lagrange afterwards answered. But these objections may give rise to this question: How comes it that, in a science in which every one admits the merit of exactness, geniuses of the first order take different sides, and continue to dispute for a long time? The reason is that in problems of this kind, the solutions of which cannot be subjected to the proof of experiment, besides the part of the calculation which is subjected to rigorous laws, and respecting which it is not possible to entertain two opinions, there is always a metaphysical part which leaves doubt and obscurity. It is because in the calculations themselves mathematicians are often content with pointing out the way in which the demonstration may be made; they suppress the developements which are not always so superfluous as they think. The care of filling up these blanks would require a labour which the author alone would have the courage to accomplish. Even he himself, drawn on by his subject and by the habits which he has acquired, allows himself to leap over the intermediate ideas. He divines his definitive equation, instead of arriving at it step by step with an attention that would prevent every mistake. Hence it happens that more timid calculators sometimes point out mistakes in the calculations of an Euler, a D'Alembert, a Lagrange. Hence it happens that men of very great genius do not at first agree, from not having studied each other with sufficient attention to understand each other's meaning.

The first answer of Euler was to make Lagrange an associate of the Berlin Academy. When he announced to him this nomination on the 20th of October, 1759, he said, "Your solution of the problem of isoperimetres leaves nothing to desire; and I am happy that this subject, with which I was almost alone occupied since the first attempts, has been carried by you to the highest degree of perfection. The importance of the matter has induced me to draw up, with your assistance, an analytical solution of it. But I shall not publish it till you yourself have published the sequel of your researches, that I may not deprive you of any part of the glory which is your due."

If these delicate proceedings and the testimonies of the highest esteem were very flattering to a young man of 24 years of age, they do no less honour to the great man, who at that time swayed the sceptre of mathematics, and who thus accurately estimated the merit of a work that announced to him a successor.

But these praises are to be found in a letter. It may be supposed that the great and good Euler has indulged in some of those exaggerations which the epistolary style permits. Let us see then how he has expressed himself in the dissertation which his letter announced. It begins as follows:

"After having fatigued myself for a long time and to no purpose in endeavouring to find this integral, what was my astonishment when I learnt that in the Turin Memoirs the problem was resolved with as much facility as felicity. This fine discovery produced in me so much the more admiration, as it is very different from the methods which I had given, and far surpasses them all in simplicity."

It is thus that Euler begins the memoir in which he explains with his usual clearness the foundation of the method of his young rival, and the theory of the new calculus, which he called the *calculus of variations*.

To make the motives of this admiration which Euler bestowed with so much frankness better understood, it will not be useless to go back to the origin of the researches of Lagrange, such as he stated them himself two days before his death.

The first attempts to determine the *maximum* and *minimum* in all indefinite integral formulas, were made upon the occasion of the curve of swiftest descent, and the isoperimetres of Bernoulli. Euler had brought them to a general method, in an original work, in which the profoundest knowledge of the calculus is conspicuous. But however ingenious his method was, it had not all the simplicity which one would wish to see in a work of pure analysis. The author admitted this himself. He allowed the necessity of a demonstration independent of geometry. He appeared to doubt the resources of analysis, and terminated his work by saying, "If my principle be not sufficiently demonstrated, yet as it is conformable to truth, I have no doubt that by means of a rigid metaphysical explanation it may be put in the clearest light, and I leave that task to the metaphysicians."

This appeal, to which the metaphysicians paid no attention, was listened to by Lagrange, and excited his emulation. In a short time the young man found the solution of which Euler had despaired. *He found it by analysis.* And in giving an account of the way in which he had been led to that discovery, he said expressly, and as it were in answer to Euler's doubt, that he regarded it not as a metaphysical principle, but as a necessary result of the laws of mechanics, as a simple corollary from a more general law, which he afterwards made the foundation of his Mechanique Analy ique. (See that work, page 189 of the first edition).

This noble emulation which excited him to triumph over difficulties considered as insurmountable, and to rectify or complete theories remaining imperfect, appears to have always directed M. Lagrange in the choice of his subjects.

D'Alembert had considered it as impossible to subject to calculation the motions of a fluid inclosed in a vessel, unless this vessel had a certain figure. Lagrange demonstrates the contrary; except in the case when the fluid divides itself into different masses. But even then we may determine the places where the fluid divides itself into different portions, and determine the motion of each as if it were alone.

D'Alembert had thought that in a fluid mass, such as the earth may have been at its origin, it was not necessary for the different beds to be on a level. Lagrange shows that the equations of D'Alembert are themselves equations of beds on a level.

In combating D'Alembert with all the delicacy due to a mathematician of his rank, he often employs very beautiful theorems, for which he was indebted to his adversary. D'Alembert on his side added to the researches of Lagrange. "Your problem appeared to me so beautiful," says he in a letter to Lagrange, "that I have sought for another solution of it. I have found a simpler method of arriving at your elegant formula." These examples, which it would be easy to multiply, prove with what politeness these celebrated rivals corresponded, who, opposing each other without intermission, whether conquerors or conquered, constantly found in their discussion reasons for esteeming each other more, and furnished to their antagonist occasions which might lead them to new triumphs.

The Academy of Sciences of Paris had proposed, as the subject of a prize, the theory of the libration of the moon. That is to say, they demanded the cause why the moon, in revolving round the earth, always turns the same face to it, some variations excepted, observed by astronomers, and of which Cassini had first explained the mechanism. The point was to calculate all the phenomena, and to deduce them from the principle of universal gravitation. Such a subject was an appeal to the genius of Lagrange, an opportunity furnished to apply his analytical principles and discoveries. The attempt of D'Alembert was not disappointed. The memoir of Lagrange is one of his finest pieces. We see in it the first developement of his ideas, and the germ of his Mecanique Analytique. D'Alembert wrote to him: "I have read with as much pleasure as advantage your excellent paper on the Libration, so worthy of the prize which it obtained."

This success encouraged the Academy to propose, as a prize, the theory of the satellites of Jupiter. Euler, Clairaut, and D'Alembert had employed themselves about the problem of three bodies on occasion of the movements of the moon. Bailly then applied the theory of Clairaut to the problem of the satellites, and it had led him to very interesting results. But this theory was insufficient. The earth has only one moon while Jupiter has four, which ought continually to act upon each other and alter their positions in their revolutions. The problem was that of six bodies. Lagrange attacked the difficulty and overcame it, demonstrated the cause of the inequalities observed by astronomers, and pointed out some others too feeble to be ascertained by observations. The shortness of the time allowed, and the immensity of the calculations, both analytical and numerical, did not permit him to exhaust the subject entirely in a first memoir. He was sensible of this himself, and promised further results, which his other labours always prevented him from giving. Twenty-four years after M. Laplace took up that difficult theory, and made important discoveries in it, which completed it and put it in the power of astronomers to banish empiricism from their tables.

About the same time a problem of quite a different kind drew the attention of M. Lagrange. Fermat, one of the greatest mathematicians of his time, had left very remarkable theorems respecting the properties of numbers, which he probably discovered by induction. He had promised the demonstrations of them; but at his death no trace of them could be found. Whether he had suppressed them as insufficient, or from some other cause, cannot now be ascertained. These theorems perhaps may appear more curious than useful. But it is well known that difficulty constitutes a strong attraction for all men, especially for mathematicians. Without such a motive would they have attached so much importance to the problems of the brachytochrone, of the isoperimetres, and of the orthogonal trajectories? Certainly not. They wished to create the science of calculation, and to perfect methods which could not fail some day of finding useful applications. With this view they attached themselves to the first question which required new resources. The system of the world discovered by Newton was a most fortunate event for them. Never could the transcendental calculus find a subject more worthy or more rich. Whatever progress is made in it, the first discoverer will always retain his rank. Accordingly, M. Lagrange, who cites him often as the greatest genius that ever existed, adds also, "and the most fortunate. We do not find every day a system of the world to establish." It has required 100 years of labours and discoveries to raise the edifice of which Newton laid the foundation. But every thing is ascribed to him, and we suppose him to have traversed the whole country upon which he merely entered.

Many mathematicians doubtless employed themselves on the theorems of Fermat; but none had been successful. Euler alone had penetrated into that difficult road in which M. Legendre and M. Gauss afterwards signalized themselves. M. Lagrange in demonstrating or rectifying some opinions of Euler, resolved a problem which appears to be the key of all the others; and from which he deduced a useful result; namely, the complete resolution of equations of the second degree, with two indeterminates which must be whole numbers.

This memoir, printed like the preceding, among those of the Turin Academy, is notwithstanding dated Berlin, the 20th September, 1768. This date points out to us one of the few events which render the life of Lagrange not entirely a detail of his writings.

His stay at Turin was not agreeable to him. He saw no person there who cultivated the mathematics with success. He was impatient to see the philosophers of Paris, with whom he corresponded. M. de Caracioli, with whom he lived in the greatest intimacy, was appointed ambassador to London, and was to pass through Paris on his way, where he intended to spend some time. He proposed this journey to M. Lagrange, who consented to it with joy, and who was received as he had a right to expect by D'Alembert, Clairaut, Condorcet, Fontaine, Nollet, Marie, and the other philosophers. Falling dangerously ill after a dinner in the Italian style given him by Nollet, he was not able to accompany his friend to London, who had received sudden orders to repair to his post, and who was obliged to leave him in a furnished lodging under the care of a confident charged to provide every thing.

This incident changed his projects. He thought only of returning to Turin. He devoted himself to the mathematics with new ardour, when he understood that the Academy of Berlin was threatened with the loss of Euler, who thought of returning to Petersburgh. D'Alembert speaks of this project of Euler, in a letter to Voltaire, dated the 3d March, 1766: "I shall be sorry for it," says he, "he is a man by no means amusing, but a very great mathematician." It was of little consequence to D'Alembert, whether this man *by no means amusing*, went 7 degrees nearer the pole. He could read the works of the great mathematician as well in the Petersburgh Memoirs as in those of Berlin. What troubled D'Alembert was the fear of seeing himself called upon to fill his place, and the difficulty of giving an answer to offers which he was determined not to accept. Frederick in fact offered him again the place of President of his Academy, which he had kept in reserve for him ever since the death of Maupertuis. D'Alembert suggested the idea of putting Lagrange in the place of Euler, and, if we believe the author of the Secret History of the Court of Berlin, (vol. ii. p. 414,) Euler had already pointed out Lagrange as the only man capable of filling his place. In fact it was natural that Euler, who wished to obtain permission to leave Berlin, and D'Alembert who wanted a pretext not to go there, should both of them without any communication have cast their eyes on the man who was best fitted to maintain the eclat which the labours of Euler had thrown round the Berlin Academy.

Lagrange was pitched upon. He received a pension of 1500 Prussian crowns, about 250*l.*, with the title of Director of the Academy for the Physico-mathematical Sciences. We may be surprised that Euler and Lagrange, put successively in the place of Maupertuis, received only the half of his salary, which the king offered entire to D'Alembert. The reason is that this prince, who at his leisure hours cultivated poetry and the arts, had no idea of the sciences, though he considered himself obliged to protect them as a king. He had very little respect for the mathematics, against which he wrote three pages in verse, and sent them to D'Alembert himself, who deferred writing an answer till the termination of the siege of Schweidnitz; because he thought it would be too much to have both Austria and the mathematics on his hands at once. Notwithstanding the prodigious reputation of Euler, we see, from the king's correspondence with Voltaire, that he gave him no other appellation than his *narrow-minded geometer, whose ears were not capable of feeling the delicacy of poetry.* To which Voltaire replies: *We are a small number of adepts who know one another; the rest are profane.* We see well that Voltaire who had written so well in praise of Newton, endeavours in this place to flatter Frederick. He enters out of complaisance into the ideas of this prince, who wished to put at the head of his academy a man who had at least some pretensions to literature. Fearing that a mathematician would not take sufficient interest in the direction of literary labours; and that a man of literature would have been still worse placed at the head of a society composed in part of philosophers of whose language he was ignorant; on that account he divided the situation, and put two persons in it, that it might be completely filled.

(*To be continued.*)

Article II.

On the Discovery of the Atomic Theory. By Thomas Thomson, M.D. F.R.S.

In the Philosophical Magazine for January, 1814, p. 54, published on the first of February, there is a paper entitled *The Discovery of the Atomic Theory claimed for Mr. Higgins,* by John Nash, Esq.; on which I conceive it to be necessary for me to make some remarks. These I should have published in the Number of the *Annals of Philosophy* for March, but could not find room for them; and in our last Number I thought there was sufficient controversial matter without any addition from me.

Mr. Nash, in the paper alluded to, quotes a note of mine from the *Annals of Philosophy,* vol. ii. p. 445, which I think it necessary to transcribe.

"The work of Higgins on *Phlogiston* is certainly possessed of much merit, and anticipated some of the most striking subsequent discoveries. But when he wrote, metallic oxides were so little known, and so few exact analyses existed, that it was not possible to be acquainted with the grand fact, that oxygen, &c. always unite in determinate proportions which are multiples of the minimum proportion. The atomic theory was taught by Bergman, Cullen, Black, &c. just as far as it was by Higgins. The latter indeed states some striking facts respecting the gases, and anticipated Gay-Lussac's theory of volumes; but Mr. Dalton first generalized the doctrine, and thought of determining the weights of the atoms of bodies. He showed me his table of symbols and the weights of the atoms of six or eight bodies in 1804; and I believe the same year explained the subject in London in a course of lectures delivered in the Royal Institution. The subject could scarcely have been broached sooner. But about the same time several other persons had been struck with the numbers in my tables of metallic oxides published in my *Chemistry*; and the doctrine would have certainly been started by others if Dalton had missed it."

Mr. Nash makes the following *assertions* by way of animadversion on this note.

1. That I have endeavoured to deprive Mr. Higgins of the honour due to the first author of the atomic theory. This theory, he says, is now generally received and admired. Sir H. Davy, Berzelius, and others, have spoken of it in terms of unqualified approbation.

2. That I have endeavoured to give the credit of the discovery to Mr. Dalton.

3. That I have disingenuously termed Mr. Higgins's book *a work upon phlogiston*, instead of *A comparative View of the Phlogistic and Antiphlogistic Theories with Inductions*, its real title.

4. That I have affirmed falsely that, when Mr. Higgins wrote, metallic oxides were too little known and too imperfectly analysed, to render it possible to found an atomic theory upon them: whereas Mr. Higgins has fully treated of this subject in page 295 of his book, and what I term in my note the *grand fact* constitutes the great leading principle which Mr. Higgins endeavours to establish throughout his book.

5. That Bergman, Cullen, and Black did not teach the atomic theory as far as Mr. Higgins; for Mr. Nash has looked over their writings without finding any traces of such a doctrine.

6. That the doctrine was first generalized by Mr. Higgins, as appears from pages 15, 37, 80, and 81 of his book.

7. That my remark, that the doctrine would have been started by others if Dalton had missed it, is most disingenuous; 1. Because I make Dalton the starter while he was only the pursuer. 2. Because the sentiment is unworthy of any scientific man, and is equally disparaging to the merit of any discovery whatever, and the best answer is to remind me of the story of Columbus and his egg.

8. Mr. Nash disclaims atrributing to my mis-statement any unworthy motive, and says he has no intention of vindicating or explaining any of Mr. Higgins's theories and positions.

These, I take it, are all the assertions contained in Mr. Nash's paper. Every reader, I presume, after perusing them, will agree that it was absolutely incumbent on me to notice them, as, if true, they affect not merely my reputation as a writer, but my character as a man. I shall examine them in succession; but I beg leave, in the first place, to notice Mr. Nash's last assertion, which seems to be tacked to his paper by way of salvo.

I must however disclaim the benefit of this salvo, for in no fewer than two places in his paper he has stigmatized my conduct as *disingenuous*. Now the word *disingenuous* in the English language means *uncandid*. A man never can be *uncandid* except from an unworthy motive. Therefore, if Mr. Nash's assertions cannot be refuted, I must be considered as guilty of acting from *unworthy motives*. Though what these motives can be, it would, I dare say, puzzle Mr. Nash with all his ingenuity to conjecture. I do not claim the discovery for myself. I have not even the honour of being personally acquainted with Mr. Higgins. I indeed saw him once, about twelve years ago, and was introduced to him at that time by Sir H. Davy. Our conversation lasted only about five minutes, and consisted merely in some information respecting the mineralogy of Ireland, which I wanted to transmit to a friend of mine in France. I have never seen him since. I am not aware of any publications of his that have appeared since that period. It could not therefore be a spirit of jealousy or revenge by which I was actuated. In short, if the note which I have quoted above was *uncandid*, I must have written it from the mere abstract love of falsehood; for no other conceivable motive can be assigned. Now I will tell Mr. Nash why the note was written. It was written solely for the information of Dr. Berzelius, who had no opportunity of seeing Mr. Higgins's book, and who I thought might be misled by the assertion of Sir Humphry Davy respecting the discovery of the atomic theory.

Let us now proceed to examine Mr. Nash's assertions one by one.

1. To the first allegation I plead guilty. I have certainly affirmed that what I consider as the atomic theory was not established in Mr. Higgins's book. And here is my reason. I have had that book in my possession since the year 1798, and had perused it carefully; yet I did not find any thing in it which suggested to me the atomic theory. That a small hint would have been sufficient I think pretty clear from this, that I was forcibly struck with Mr. Dalton's statement in 1804, though it did not fill half an octavo page: so much so indeed, that I afterwards published an account of it; and I still consider myself as the first person who gave the world an outline of the Daltonian theory. I beg leave to put Mr. Nash right respecting Sir H. Davy and Dr. Berzelius. Neither of them has adopted what I conceive to be the atomic theory. Sir H. Davy has written two very violent and I think indecorous notes against it. I admit indeed that he has adopted a theory which comes nearly to the same thing; but he vehemently disclaims the term *atomic*. Had Mr. Nash read the admirable paper on the cause of chemical proportions in the second and third volumes of the *Annals*

of Philosophy, he would have seen that Berzelius likewise rejects the atomic theory, and substitutes in its place what may be termed the *theory of volumes*.

2. The second allegation is likewise true. I have given the credit of the discovery to Mr. Dalton, because I thought and still think, that the generalization (which constitutes the discovery) was made by Mr. Dalton.

3. As to the third allegation I make this answer. The copy of Mr. Higgins's book which has been in my possession since 1798, is titled on the back *Higgins on Phlogiston*; and it was so titled when it came into my possession. The disingenuousness then, of which Mr. Nash complains, must be ascribed not to me, but to the bookseller, or author, or whoever put the title on the back of the book. I merely copied a title ready made out to my hand. After all, the name seems to me not misapplied. The book appears to have been written as a refutation of Kirwan's Treatise on Phlogiston, and I consider it as a very complete one. This probably suggested the title on the back. I beg leave to put Mr. Nash right about the date of the publication of Mr. Higgins's book. It was not published as he says in 1790, but in 1789. When a man accuses another of *inaccuracy* and *disingenuousness* he ought to be very precise in his dates: otherwise he puts it in the power of his antagonist to retort the accusation.

4. The fourth assertion of Mr. Nash has astonished me more than all the rest; and I have been irresistibly led to draw from it the two following inferences: 1. That Mr. Nash is totally unacquainted with the history of chemistry during the last thirty years. 2. That he has never perused Mr. Higgins's book; but has merely written what somebody else has dictated, and written it inaccurately.

The composition of metallic oxides was entirely unknown till Lavoisier published his dissertation on the subject in 1778, and it was seven years more before that philosopher could gain a single convert to his opinions. The first attempt at the analysis of metallic oxides was made by Lavoisier in his paper entitled Experiments on the Precipitation of one Metal by another, published I believe in 1785. When Mr. Higgins wrote in 1789, this was the only set of experiments that had appeared on the subject. Now the following table exhibits the results of Lavoisier's experiments. Mr. Higgins gives the same table in page 268 of his book, but in a more mutilated form, and obviously copied at second hand from Kirwan. But I choose to give Mr. Lavoisier's own table without any curtailment, that the case may be stated as strongly in Mr. Nash's favour as possible.

Oxygen combined with 100 *Metal.*

METALS.	By precipitating the metals by one another.	By calcination in the open air	By detonation in nitre.	By combination with arsenic.	By solution in acids.
Platinum	81·690				
Gold	43·612				
Iron (as black oxide)	27·000	3·000	..	3·000	
Ditto (as red oxide)	37·000	..	40·000		
Copper	36·000	14·245	10·000	6·667	15·85
Cobalt	29·190				
Manganese	21·176				
Zinc	19·637	19·328			
Nickel	16·875 12·568				
Antimony	13·746	14·000	30·000	..	22·383
Tin	14·000	17·450	16·233	10·764	23·555
Arsenic	11·739 24·743				
Silver	10·800				
Bismuth	9·622	7·750			
Mercury	8·000	7·750			
Lead	4·470	9·000	..	..	14·190

Mr. Higgins gives only the first and last columns of this table; nor does it appear, from any thing that I can find in his book, that he knew any additional facts Now I appeal to every person that has the least information on the subject, whether it be possible from this table to form any conception of the existence of the atomic theory, or with what I call the *grand fact* in my note, that metals unite with various doses of oxygen, always bearing to each other the ratios 1, 2, 3, &c. I do not believe that Mr. Higgins will pretend that he possessed any such knowledge at the time; but if he should, I am very ready at any time to demonstrate, from several passages in his book, that he possessed no such knowledge.

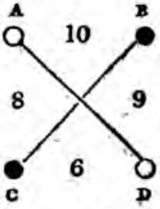
Mr. Nash refers to page 295, &c. of Mr. Higgins's book, and says that I shall find the subject treated of there at full length. Now I deny that there is one word upon the subject of metallic oxides in page 295, or from page 295 to the end of Mr. Higgins's book. He treats of the distillation (or analysis, as he chooses to call it,) of a urinary calculus, and of nothing else, from page 283 to the end of the book.

5. I was a good deal amused with Mr. Nash's fifth assertion. It shows very clearly that he is but little, if at all, acquainted with the philosophical opinions of Cullen, Black, and Bergman. When I published my paper on the atomic theory in the seventh Number of the *Annals of Philosophy*, I received a letter from Dr. Pearson, complaining of my conduct in not noticing the opinions of Cullen, Black, and Fordyce, on the subject, and affirming that these philosophers had taught the atomic doctrine, and that he himself had learned it from them, and had taught it always from the beginning just as far as I had explained it. Now I must own, with all due deference to Dr. Pearson, that this in my opinion was going

a little too far. The science of chemistry had not advanced far enough, and too few accurate analyses were known, to admit any attempt to determine the weight of atoms. This was the case also at the time that Mr. Higgins wrote; and I am not aware of any attempt in his book to determine the weight of a single atom: but Cullen, Black, and Bergman, had made some steps towards the atomic theory, and these by no means unimportant ones.

The chemical works of Dr. Cullen might soon be looked over; for I am not aware of any thing which he wrote on the subject except a few pages on the cold produced by the *evaporation of ether*. My knowledge of his opinions was derived from the late Professor Robison, of Edinburgh, who had the means of information, and, as he was a particular friend and great admirer of Dr. Black, was entitled to credit. Now he informed me that Dr. Black's explanation of double decompositions, which he annually gave in his Class, had been originally broached by Dr. Cullen. This was the circumstance that induced me to introduce Dr. Cullen's name along with Black and Bergman. There is one man still alive who is probably acquainted with Dr. Cullen's chemical lectures, and with the materials which they furnished to Dr. Black; I mean Mr. Watt, of Birmingham.

As to Dr. Black, I consider myself as acquainted with his opinions, because I attended his lectures; and there are thousands in Great Britain who did the same, and who cannot but recollect the facts that I shall state. Dr. Black taught that bodies combine in definite proportions, and he explained double decomposition by means of diagrams, not indeed the same as those of Mr. Higgins, but much simpler, and more elegant. I have been informed by Professor Robison that he employed these diagrams from the very beginning of his career as a Professor. One of them is given in page 544, vol. i. of the printed edition of Dr. Black's lectures. I have no doubt that all similar diagrams published in London by Fordyce, &c. were derived from this original source. Nay, I even suspect that the diagrams of Mr. Higgins himself might be traced to the same origin. Now could the doctrine of definite proportions be taught, and could double decomposition be explained, in this way (for I quote Dr. Black's explanation)? Let the bodies A and B be united with a force 10, and the bodies C and D with a force 6; suppose the attraction of A for C to be 8, and that of B for D to be 9, if we mix these bodies A will unite with C, and B with D—I say could these opinions and explanations be given by any one who did not believe the atomic theory, at least to a certain extent? To me they conveyed just as much of the atomic theory as the perusal of Mr. Higgins'sbook did.



As to Bergman, the perusal of his Treatise on Electric Attractions, particularly his explanation of the apparent anomalies, his account of double decompositions, and the numerous diagrams of double decompositions which he has given in the first plate of the third volume of his Opuscula, demonstrate irresistibly that he entertained the same opinions on the subject with Dr. Black. Indeed I believe, when Mr. Higgins wrote, these opinions were universally admitted by chemists as first principles: nor do I conceive that he was aware that any thing new could be deduced from his writings till many years afterwards. The first person who formally opposed himself to the atomic theory, and whose opinions are incompatible with it, was Berthollet.

Had Mr. Nash been acquainted with these facts, he would have, I presume, expressed himself on this subject with a little more decorum and modesty than he has thought proper to do.

6. Mr. Nash's sixth assertion will make it requisite for me to state distinctly how far the generalization of the *atomic theory* is to be found in Mr. Higgins's book. Now there are four remarkable passages in the work in question, which I considered, when I wrote my note, as referring to the theory of volumes. But upon reconsidering them, I conceive that they may be applied likewise to the atomic theory; and indeed there is one of the four that applies to that theory without any ambiguity. It is probable that Mr. Nash alludes to three of these four passages in the pages to which he refers; but the fourth, which I consider as the most important of all, he has entirely overlooked. In justice however to Mr. Higgins, I think it requisite to bring forward every thing that can be urged in favour of his claim. The passages are the following. I shall not use Mr. Higgins's words, but give his meaning in the modern language of chemistry.

Page 36.—1 ultimate particle of sulphur and 1 of oxygen constitute sulphurous acid; 1 ultimate particle of sulphur and 2 of oxygen constitute sulphuric acid.

Page 37.—Water is composed of 1 ultimate particle of oxygen and 1 ultimate particle of hydrogen.

Page 81.—Sulphureted hydrogen is composed of 9 ultimate particles of sulphur and 5 of hydrogen.

Page 132.—The composition of the compounds of azote and oxygen are as follows:—

	Ultimate	Particles.
Nitrous oxide................	1 azote	+ 1 oxygen
Nitrous gas	1	+ 2
Red nitrous vapour	1	+ 3
Straw coloured nitrous acid	1	+ 4
Nitric acid..................	1	+ 5

Now from these passages, which are all I can find in the book relating to the subject in question, Mr. Higgins is entitled to affirm that he had an idea of the atomic theory when he wrote his book: but he is not entitled to affirm that he taught it; or that any reader, merely from perusing his book, would have formed an idea of the atomic theory. There is not a single attempt at generalization, no allusion to the rate at which the quantity of oxygen increases, no statement of the weight of an atom, or any reference to such a weight. The mere statement of such numbers as Mr. Higgins gives in the preceding passages is not alone sufficient to constitute a man the discoverer of the atomic theory, otherwise the number of competitors would become very considerable. Mr. Chenevix gave two similar examples. He found the oxides of platinum composed of

100 metal + 7·5 oxygen
100 + 15·0

and the oxides of copper composed of

100 metal + 13 oxygen
100 + 25

Yet these facts, which are as striking as any given by Mr. Higgins, did not constitute Mr. Chenevix the discoverer of the atomic theory. In the second edition of my System of Chemistry I gave a considerable number of similar examples: yet I do not, on that account, lay any claim to the discovery of the atomic theory. The generalization, both in the case of Mr. Chenevix and myself, as well as in the case of Mr. Higgins, was wanting. And whatever may be the opinions of Mr. Nash on the subject, it is this generalization, for which we are solely indebted to Mr. Dalton, that constitutes the whole merit. Richter had gone still further; for he had generalized and shown the constancy of the proportions in which acids and bases unite with each other in all cases. He certainly therefore has some claim to at least a share of the merit of the atomic theory. But as I have never had an opportunity of reading his book, I cannot pretend to estimate how far his merit extends. To judge from the papers of his, on the subject, in Crell's Annals, he seems to have been misled by some mystical opinions respecting the figurate numbers, and to have had no notion whatever of the atomic theory.

All that Mr. Higgins then can allege, is, that he was acquainted with the atomic theory when he published his book, but thought proper to keep it a secret; and would in all probability have kept it a secret for ever, had not the discovery been made by Mr. Dalton. Finding the theory before the public, and finding its importance daily increasing, it would appear that now, after an interval of twenty-five years, he thinks it worth his while to put in his claim.

Whether Mr. Higgins was actually in possession of this theory twenty-five years ago is a question which we have no means of deciding; nor do I conceive it to be a point of much consequence. We can easily see that if he knew it, he at least thought proper to keep his own secret. Now it is a law in the republic of letters, and I conceive a very proper one, that if a man makes a discovery, and conceals it, if the same thing be discovered and published by another person, the original discoverer loses all claim to the honour accruing from the original invention, unless he demonstrate to the satisfaction of the public that he was in possession of it first.

At the same time there are several passages in Mr. Higgins's book which lead me to suspect that his knowledge of the atomic theory was not quite so steady and precise as Mr. Nash would have us believe. I shall quote a few of these passages, that Mr. Nash may have an opportunity of exercising his ingenuity upon them. And I would advise him, as a friend, before he ventures to appear again in the field, to peruse Mr. Higgins's book carefully. He will perhaps find that some of Mr. Higgins's peculiar opinions are more intimately connected with the atomic theory than Mr. Nash seems at present to suspect.

Page 7.—Seven measures of oxygen form 5 or 4½ measures of carbonic acid gas.

Page 157.—Sulphur will not unite with much more oxygen than is sufficient to convert it into sulphurous acid; but in time, by help of water, heat, and air, it absorbs a sufficient quantity to form perfect sulphuric acid.

Page 159.—Nitric acid may absorb more oxygen.

Page 257.—Some metals absorb more oxygen than they can retain when united to acids, and therefore lose a portion when dissolving in nitric acid.

Page 263.—If the precipitant cannot take up the whole of the oxygen, it (*the metal*) is thrown down in a semireduced state.

Page 265.—A very small quantity of oxygen over and above a certain portion will render some metals quite insoluble.

Page 270.—Platina is soluble in various proportions of oxygen.

Page 274.—Acid bases retain oxygen with less force when fully saturated with it, than when united to a small portion.

These passages are not transcribed verbatim, but translated into the modern language of chemistry, and I have always carefully given the exact sense. I myself conclude from them, that Mr. Higgins never had generalized the atomic theory. But this is a point of little consequence. Respecting the *great fact*, that he did not communicate the atomic theory, there can be no doubt.

7. Mr. Nash's seventh assertion consists of two parts. The first part has been already sufficiently noticed. In answering the second, it becomes necessary for me to state to whom I alluded, when I said that the doctrine would have been certainly started by others if Dalton had missed it. I alluded to Dr Wollaston. I had the means of knowing that this gentleman had been struck with the proportions of oxygen in my table of metallic oxides, and that he had begun to study the subject when my account of the Daltonian theory prevented him from proceeding with his investigations. Now Mr. Nash must be very little acquainted with the sagacity of this philosopher if he can entertain any doubt that the result of his investigation would have been the discovery of the atomic theory.

I might mention the name of another gentleman, who I know was engaged in the same investigation; but I conceive it to be unnecessary. How it should be unworthy of a scientific man to believe that there are other people in Great Britain capable of discovering the atomic theory besides Mr. Dalton or Mr. Higgins (if Mr. Nash chooses to substitute that name) I do not understand. But even if it should be ever so unscientific, I must still believe it; because my knowledge of Dr. Wollaston's sagacity prevents me from entertaining any doubt on the subject. Nay, I will go further than that, and say that I believe if Newton never had existed we should at this moment have been in possession of the theory of gravitation. Nature has not been so stinted in her gifts as to confine the faculties of invention and discovery to one individual, or one nation, or one class of society.

I have now considered the whole of Mr. Nash's assertions, and leave the unbiassed reader to draw his own consequences. Perhaps I ought to apologize for the length of this article; but I could not well lay the subject before the public in less space. I take it for granted that nothing which Mr. Nash may afterwards publish in his own vindication can render it necessary for me to resume the subject.

Article III.

On the Limits of perpetual Snow in the North. By Leopold Von Buch, Fellow of the Royal Academy of Sciences in Berlin.

(*Concluded from* p. 220.)

If we advance ten degrees farther north than this latitude to the remotest cliffs of the continent of Europe, in latitude 70° and 71°, we need not be surprised in so remote a situation, and so much nearer the pole, to find the snow line not rising to any great height above the surface of the earth. From the accounts of the violence of the Lapland cold, one would readily believe that the snow line does not rise much higher than the surface of the sea. But the first appearance of the land shows us that this is far from being the case. For even in the 70th degree of latitude, all cultivation is not at an end. We still find gardens and corn fields, villages at the mouth of the torrents, and forests in the valleys. *Altengaard*, the residence of the bailiff in the interior of *Altenfiord*, as it appears in summer, would even in this climate be called rich. It lies in the middle of a tall fir wood, with magnificent views, on the other side of the fiord, of rocks and snow mountains. Through the valley rushes the mighty stream, *Elvebacken*, which is the means of supporting at least 20 houses, that lie in the midst of its fields and meadows. Who can here think of the snow line? The firs cover the neighbouring *Kongshavnfieldt* to the very top, and it is 560 English feet high. In this place we first succeed in finding the limit of the firs. On *Skaanevara*, which is 1408 English feet in height, and on *Borrisvara*, this tree first disappears when we have got to the height of rather more than 746 English feet. But before this mountain can be covered with perpetual snow, it must rise to a greater height than it does. Snow is not to be seen in summer upon any of the mountains in the immediate neighbourhood of *Altengaard*.

The mountains of *Talvig* are higher. They lie two German miles from *Altengaard*. For *Talvig* is situated at the foot of the last branch of the great *Kiolengebirge*, which running over a great tract of country separates Sweden from Norway; but at this place breaks into fragments, and passing from *Sverholt* and *Nordkyn* on the main land, goes to the *North Cape* in *Mageroe* and the Cape of *Porsanges*.

The first rocks above the creek of *Talvig* rise with uncommon rapidity, and the brooks flowing from above form foaming waterfalls; but after ascending about a thousand feet, mountain valleys make their appearance, and we ascend for some miles very slowly. As the elevation increases, the Lapland vegetation, with which we had become familiar in the valleys, disappears under our feet. The firs soon vanish altogether, and the birches become continually smaller. At last they disappear entirely, and between tufts of mountain grass and dwarf birches we perceive prodigious quantities of berry bushes spreading themselves; bilberries (*vaccinium myrtillus*) on the dry heights, and cloudberries (*rubus chamæmorus*) in marshy places. When we ascend higher, the bilberries cease to bear fruit, they stand single with few leaves, and no longer collected together in the form of bushes. They disappear at last, and the mountain grass soon follows them. Now the dwarf birches brave the height and the cold; but they also disappear before we reach the line of perpetual snow, and there remains a broad border surrounding this line, on which, if we except mosses, only a few small plants support themselves with difficulty. The reindeer moss itself, which in the woods rivals the bilberry in luxuriancy of growth, springs up but sparingly at such heights. On the top of the mountain, which is almost a plain, there is no ice and no glacier to be seen. But the snow never leaves these heights. For a few weeks only may some of the summits be seen free from their snowy covering.

Akka-Solki is one of the most remarkable of these summits, though it rises but a little way above the mountain plain. Only two or three other conical summits in this neighbourhood interrupt the extensive prospect. On the 16th of August, 1807, the snow had left this height for a few days only, and the black soil upon the declivity of the mountain could be distinguished. The barometer stood

At *Akka-Solki* at .... 26·564 English inches.
Therm. 51·69°
At *Talvig*, 70 feet above the sea, at.. 29·912 inches
Therm. 61·25°

This gives us the height of *Akka-Solki* above the sea 3358 English feet. The highest mountain in this place lies at no great distance south east. It is separated from *Akka-Solki* by the deep valley of *Storvands*, and is only about 160 feet higher, or 3518 feet above the level of the sea. On this mountain the snow lies all the year round, and it may be seen from *Altengaard* always covered with snow. If an extensive plain were to stretch itself at the height of *Storvandsfieldt*, it is clear that it would be always covered with snow, even during the height of summer, and probably glaciers would be produced on the declivity over the *Fiord*. The height of the snow line then on the mountains of *Talvig* in the 70th degree of latitude is 3518 feet above the level of the sea.

Glaciers are not wanting in this country. There appear upon the north side of *Alt-Eid* some low tongues of land, over which the road from *Quænananger* to the Altensfiord passes. There a high cliff, the *Jockuls-Fieldt*, rises quite perpendicularly from the *Jockulsfiord*, and continues at the same height for about four German miles. Perpetual snow covers the sides of this mountain in uninterrupted masses, as upon the mountains of *Folge-Fonden* and *Justedals*. When we stand on the rocks above *Alt-Eid*, this snow appears like a white covering thrown artificially over the black rocks. We can see very distinctly how the glaciers in the high valleys separate from the snow, and how they tumble down towards the deep surrounding *Jockuslfiord*. In the middle over the steep, almost perpendicular, rocks, prodigious masses of ice may be seen hanging; and in summer these are perpetually falling into the *fiord*, often in such quantities, and with such violence, that in consequence of the agitation produced in the sea, the water, even at the distance of miles, rises many feet over the land, and not unfrequently washes away with it the huts of the Laplanders. *Jockuls-Fieldt*, to which the old Norwegian name *Jokull* has been given, is scarcely 3730 English feet above the level of the sea. In this place likewise, on account of the great extent of the snow, and the cold produced in consequence, the snow line is sunk below its true level.

The different heights at which the trees and bushes disappear upon the mountain of Talvig, are not accidental. All the way from Drontheim to this place, I had observed a striking regularity in this respect. It is true that the absolute heights to which the spruce firs, Scotch firs, and birches ascend, differ considerably in different latitudes; but the differences between these respective heights are every where the same. The following table exhibits the heights at which different plants disappear upon *Talvig*.

	English feet.
Scotch fir (*pinus sylvestris*)	778
Birch (*betula alba*)	1580
Bilberry (*vaccinium myrtillus*)	2033
Salix myrsenites	2152

The *salix lanata* goes higher, and reaches almost to the snow line.

	English feet.
Dwarf birch (*betula nana*)	2745
Snow line	3517

Hence the perpendicular distance between these respective heights is as follows:

	Eng. feet.
From the place where the Scotch firs disappear to the line of birches	802
From the line of birches to that of dwarf birches	1165
From the line of dwarf birches to the snow line	772
Distance of the Scotch firs from the snow line	2739
Ditto of the birches	1937

The same distances exist in every part of the Norwegian coast. If the line of firs be elevated to the height of 3000 feet, then the birches rise to 3802 feet, and the snow line is situated at an elevation of 5739 feet. The same differences probably exist over the whole surface of the earth. Hence it is not the soil which occasions these lines of elevation, but the temperature; and it determines it with such precision, that we cannot without admiration observe upon so many mountains of this coast, how the spruce firs, the Scotch firs, and the birch, appear to be cut off by a determinate horizontal line. They have reached the medium temperature of their growth, and it is not permitted them to ascend higher.

This would furnish us with an excellent method of determining the snow line directly, even when it is not in our power to ascend so high up into the atmosphere, if it were not that the growth and flourishing of trees in many cases depend more upon the length and intensity of the summer, than upon the mean temperature of the place. If it were not for this difficulty, a single moderately high mountain would enable us to determine the height of the snow line in any country. A mountain from which the laurels and cypresses had disappeared, would enable us to determine how far we must ascend to reach the line of chesnuts, then that of filberts, beeches, oaks, spruce firs, Scotch firs, birches, and last of all the snow line. In this manner we might by immediate observations determine the curve of the snow line in various meridians, and finally establish the law of the variation of temperature over the whole surface of the earth.

V.

Almost a degree north from *Alten*, and just in the neighbourhood of the great Ocean, lies *Hammerfest*, upon the island *Qualoe*, at the north end of Altenfiord, the farthest north town in the world.

How complete is the alteration here in the climate and appearance of the country. Here no trees are to be seen, and no vegetables can be raised in the gardens. The birches are only bushes, and disappear completely at the height of 746 English feet above the level of the sea. At *Alten* they rise to the height of 1500 feet. The sun very seldom is visible from these islands. The summer is destitute of heat, and scarcely two or three tolerably warm days are enjoyed. The north-west wind in the twinkling of an eye covers the surface of the earth with clouds from the sea; torrents of rain tumble down, and the clouds all day long hover upon the surface of the earth. Deeper in the fiord only light and passing showers fall; and at Alten the sky is clear and the sun shines; but a black and dusky band of clouds is always seen in the northern horizon.

Not less remarkable is the eternal mist which hangs above *Mågeroe* at the North Cape, in the 71st degree of latitude. Here we find nothing among the rocks which is entitled to the name of a bush. We perceive indeed between the cliffs a deep valley, which is sheltered from the sea wind. In it there appears here and there a remainder of birches, not in the form of bushes, but creeping along the ground. But these melancholy remains do not go higher than 430 feet. The snow line therefore, which at *Alten* was 3517 feet above the level of the sea, at *Hammerfest* is only 2345 feet high. Thus from *Fillefieldt*, as we advance towards the pole, to *Alten*, a distance of 10° of latitude, the snow line has only sunk 2025 English feet; but from Alten to the North Cape, a distance of only 1¼ degree of latitude, it has sunk 1172 feet. So great is the difference between the interior of the bays and their outlet into the sea. The vapour at its maximum above the sea falls down in the state of fog, rain, or perpetual cloud, whenever the temperature is the least diminished above these islands. In the interior of the country, so much of the vapour has already fallen to the ground, that the temperature is capable of maintaining the air transparent. The sun penetrates through the clouds, acts upon the soil, and warms it. The temperature of the atmosphere is in consequence considerably elevated, and now the sea winds drive the clouds in that high temperature as into an abyss. Scarce have they reached these regions, when they disappear altogether, and sunshine continues often for weeks without the least interruption. The interior of the *fiord* now enjoys the benefit of the warmer sea air, but the fog, which prevents the action of the sun's rays, does not penetrate so far. Hence it happened that the mean heat of the month of July (1807) at Alten was 62·4°, while at the North Cape, at the end of July and beginning of August, it was only 51·5. Hence it happens that at *Alten*, at *Reisfiord*, at *Lyngen*, almost under the 70th degree of latitude, corn is cultivated with profit, while in the island of *Tromsoe* in the same latitude, even birches vegetate with difficulty. Hence it happens that at *Lyster*, in *Sognedal*, and at *Kopanger*, in the interior of the *Sognefiord*, and in the 61st degree of latitude, not only excellent crops of wheat are raised, but all kinds of apples, pears, and cherries ripen in abundance; while in the same latitude at the mouth of the *fiord*, corn can be raised with difficulty, and garden-stuff not at all.

At the height of the snow line, this annihilation of the summer on the sea shore shows itself immediately. For the height of this line depends entirely upon the sum of the heat of the snow melting months, and not upon the cold of winter, nor the mean temperature of the year. Otherwise it could not at the North Cape be so much lower than at *Alten:* for the mean temperature of Alten is by no means so high as that of the North Cape. At *Alten* mercury often freezes, at the North Cape never. At *Alten* we frequently see the thermometer standing at — 13°, at the North Cape very seldom below 10°, or 5°; and 0 is the extreme. Accordingly, the sea does not freeze in the neighbourhood of the North Cape. When we go 20 or 30 German miles out to sea in winter, we see ice islands at a distance.

Still more: if the general annual temperature determined the height of the snow line above the surface of the sea, then in *Uleoborg*, and still more at *Torneo*, in the 65th degree of latitude, it ought not to be higher than at *Mageroe* in the 71st degree. And yet what a difference between the nature of these places? And how different likewise is the temperature of the summer, and of those months which can have any influence on the height of the snow line!

If we compare the observations made by Father Hell from the winter of 1768 to June 1769, at *Wordohuus*, a place even colder than the North Cape, with the observations of Mr. Bayly in *Kamoefiord* on *Mageroe*, and of Mr. Jeremiah Dixon at *Hammerfest*, as they observed the transit of Venus in 1769, in these places; and if we join to them some of those observations which I was enabled to make during my twelve days' residence at the North Cape, we shall find the monthly temperature in that place nearly as in the following table.

January	22·08° Fahr.
February	23·16
March	24·78
April	30·00
May	34·07
June	40·145
July	46·625
August	43·25
September	37·625
October	32·000
November	25·75
December	25·72
Mean for the year	32·100

The observations of Mr. Julin at *Uleoborg*, in the 65th degree of latitude, published in the Memoirs of the Swedish Academy for 1789, page 121, after the reductions and alterations to be afterwards stated, which I consider myself as warranted to make, give us the following mean temperature of the months in that place.

January	7·630° Fahr.
February	14·308
March	14·203
April	26·145
May	40·899
June	55·184
July	61·560
August	56·673
September	46·490
October	38·732
November	22·651
December	13·595
Mean for the year	33·172

If we compare these numbers with the preceding table, we shall find that there is very little difference between the mean annual temperature at Uleoborg and at the North Cape. Yet the mean heat of the months above the freezing point at Uleoborg amounts to 49·928°, while at Mageroe it is not higher than 39.312°. By this difference is the height of the snow line regulated; and notwithstanding the severity of the winter, it rises at Uleoborg to a considerable height.

These circumstances render the snow line of still greater importance to us. As it depends entirely upon the heat of the snow melting months, its height becomes a measure of the quantity of living beings in the places where we survey it; for the number of living beings is determined by the height of the thermometer above the freezing point. Below that point vegetables will not grow, and animals support themselves with difficulty. The thermometer in Siberia may show a degree of cold and a winter temperature lower than is known in any other part of the Continent; the medium temperature of *Jakutsk* may be 6·75° below the freezing point; and yet the trees in the country show that the snow line there is higher than at *Alten*, and probably even higher than at *Torneo*. And we may assure ourselves that in such summers both the vegetables and animals of *Torneo* will thrive. But what shall we say of *Iceland*, where the inhabitants spend their winter in their houses without fire, and yet in the 65th degree of latitude the height of the snow line is only 3086 feet above the level of the sea.*

Swedish Lapland to the south of Alten, and especially the southern parts of West Bothnia, becomes more adapted for vegetables and animals, in consequence of the heat of its summers, than the mildness of climate in general. It is true that when we go straight to Torneo, the hight of the snow line cannot be directly observed in any part of the way; for between Alten and Torneo, there is not only no mountain which rises to that limit, but in reality no mountain whatever. The Norwegian range here becomes flat, excepting that here and there a small eminence rises to the height of 400 or 600 feet. The place which limits the streams running into the Frozen Sea and the gulf of Bothnia, is only 1380 English feet above the level of the sea. We have indeed passed the *Kiolengebirge* before we come to *Kautokeino,* although the *Altens Elo* runs by Kautokeino, and falls into the Frozen Ocean. This river makes its way through a channel in the mountain, as the Rhone does in Switzerland. On the sides of the declivities in Sweden, appear by degrees, and in increasing perfection, those trees which we lost on the coast. Scotch firs show themselves again at Lippajärfwi, 1276 feet high: and at *Palajvensuu,* at the height of 1070 feet, they are as flourishing as at Alten. At Alten they disappear at the height of 746 feet: a difference which is the effect of 1¼ degree of latitude. Some miles lower down, at *Songa Muotka,* in the 68th degree of latitude, and at the height of 842 feet, the first spruce fir makes its appearance. More are soon to be met with, at first with frosted and blasted branches; but from *Muonioniska,* which is 723 feet above the level of the sea, they appear in full vigour. By degrees on the banks of the streams they form almost impenetrable woods, and a mixture of other trees and shrubs may be distinguished among them; especially the *salix pentandra,* which may be called the Lapland rose, and the aspen. In the neighbourhood of Kangis, not far from the polar circle, these woods are employed in heating iron furnaces. At last at *Pello* we come to the polar circle, where the country has become classical, by two successive measurements of a degree of latitude. Here the almost uninterrupted rows of villages all the way to Torneo show us what the climate is capable of. Corn fields come into view, and the woods retire to a distance. The woods rise to the very tops of the mountains: nothing here in summer puts us in mind of the severity of winter.

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* At *Osterjockul*, according to the observations of Lieutenants Olafsen and Wetlefsen, communicated to me by Mr. J. R. Bügge.

At *Pullingi* near *Svanstein,* the spruce firs are 213 feet below the Scotch firs. Now *Pullingi,* the highest mountain between Torneo and the polar circle, is 855 feet above the river,* and 1114 above the sea. The distance between the spruce and Scotch firs is 639 feet; the Scotch firs are 2739 feet from the snow line. This gives us the height of the snow line at the polar circle and at Torneo 4492 feet above the level of the sea. Yet the experiment of freezing mercury did not fail at Torneo.†

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* **Hermelin's Mineral Historia ofwer Lapmarken och Wester Bottn. p. 69.**

† **Hellant. Svensk. Vetensk. Acad. Handl. 1760. 312.**

VI.

There still remains a great blank in my observations; I mean the depression of the snow line between *Fillefieldt* and *Talvig*. It is no small pleasure to me to be able to fill up this blank by means of a set of very accurate observations. Dr. George Wahlenburgh, of Upsala, Fellow of the Royal Academy of Stockholm, to whom science lies under so many obligations, travelled as a botanist in that country, and in 1807, by means of excellent instruments with which he was furnished, determined the height of the great ice mountain of *Kiolengebirges*. He has consigned his important observations in a treatise which has been published in Sweden, embellished with views and maps, by the care of Baron Hermelin, to whom we lie under such obligations for our knowledge of the whole of Sweden.* Dr. Wahlenberg found the highest mountain on the north side of the polar circle, between Norwegian *Saltensfiord* and the Swedish settlement *Quickjack*, in the westernmost part of *Lulen Lappmark*. The barometer stood on the south side of *Sulitelma*, which he had ascended on the 14th of July, 1807, at 24·387 English inches: the thermometer at 42·8°. At the same time I saw a corresponding barometer at the level of the sea stand at 29·992 inches; the thermometer at 55·4°. This gives us the height of Sulitelma 5675 English feet above the sea.† The mountain rises a great way into the region of perpetual snow; and in the hollow between it and a mountain on the north not quite so high, there appears a magnificent glacier, not very steep, but of enormous breadth. It extends to *Lairo*, a full Swedish mile, a place which is still situated upon the high mountainous country. The Laplanders graze the whole summer with their reindeer upon the edge of this glacier. They call it *Lairo geikna*; for in Lapland the name *geikna* or *jakna* is applied in the same way as the Iceland word *jockal*, the Norwegian word *jisbrae*, the Tyrolese word *ferner*, and the Swiss word *gletcher (glacier)*. With Sulitelma begins a range of ice mountains, which extends for a whole degree of latitude, and unites with the steep *Ridatjock* above the *Tysfiord*. Here there are many mountains whose name terminates in *geikna*, from all of which glaciers proceed. But this is the only place in the north where glaciers are frequent. On the south side they are not to be met with till we come to the mountains of *Justedal*, in the 62d degree of latitude.

Wahlenberg carefully examined the height of the snow line on this mountain; and the reader will not be a little surprised when he learns that according to his observations this line is not higher than 3837 English feet above the level of the sea, scarcely higher than in the interior of Fenmark's *fiords*. *Podnak*, which is scarcely free from snow, is not elevated higher than 3698 feet, and by no means so high as *Lairo*. We might ascribe this appearance to the action of the glaciers; but the birches and firs in this place give no higher elevation to the snow line. In *Saltvattudal*, at *Saltensfiord*, the birches disappear at the height of 1812 feet, and the Scotch firs only ascend a few hundred feet above the valley. Though it be not improbable that the vast mass of ice on *Sulitelma* may sink the snow line somewhat, yet it is evident that the lowering of this line on the north side of the polar circle to the 70th degree of latitude is very small, and bears no proportion to the rate at which it is lowered at the 60th degree of latitude.

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* Berättelse om mättningar för att bestämma Lappska fjällens hogd och Temperatur. Stockholm, 1808. With a map, and three Alpine views.

† Dr. Wahlenberg, in his instructive book, gives the height 5513 feet; but as the corresponding barometer at Altengaard stands accidentally 0·125 inch below the Swedish barometer, as I afterwards ascertained by different observations, I have corrected this difference in the statement which I have given in the text.

It were to be wished that we had observations by means of which we could compare the height of the snow line in the 61st degree of latitude with its height at the polar circle. But such observations are wanting. Even what we know of the height of the snow line in the 62d degree of latitude is not very certain. It is, true that Mr. Esmark has had the courage to climb to the top of *Sneehatten* in that latitude, the most elevated mountain in Scandinavia, which no other person has ventured to ascend, either before him or after him. From accurate observations made upon the top of this mountain, compared with corresponding observations made by Provost *Pihl* at *Vang* in *Hedemarken*, he found its height 8121 English feet above the level of the sea. But we do not know at what height on that mountain the snow line is situated. I saw on the north side of *Dovrefieldt* the Scotch firs first make their appearance at *Drivstuen* at the height of 2451 feet. This would make the height of the snow line at that place 5190 feet. The highest part of the road over *Dovrefieldt*, between *Jerkin* and *Kongsvold*, lies 4556 feet above the level of the sea, and it does not reach the snow line. *Herebacken*, between *Fogstuen* and *Tofte*, is 4575 feet above the level of the sea. This place likewise is free from snow in summer.

When we collect together all these facts, we obtain at last the following results for the height of the snow line in the north, and on the Norwegian chain of mountains.

Its height is in latitude	61°	5542 Eng. feet
	62½	5180
	67	3837
	70	3517
	71	2345

It is evident that we cannot, by endeavouring to ascertain the curve which this snow line forms, apply our observations made on one meridian to determine the height of the snow line in other meridians. Observations made in the interior of Norway cannot be compared with those made in Iceland; neither can the Siberian be compared with the Norwegian. But it is probable that the height of the snow line at *Mageroe* would form a point in the Icelandic curve; for Iceland and Mageroe lie under similar meteorological meridians.

Some Additional Observations.

1. *Respecting Mr. Julin's Observations of the Thermometer at Uleoborg, noticed in page 344.*

As *Uleoborg* lies in the 65th degree of latitude, a collection of 12 years' observations in such a place must be very valuable, and deserve some pains to make them as accurate as possible.

The first observations, says Julin, from 1776 to 1782, were made by his predecessor, the apothecary Kerborg, with a Florentine spirit of wine thermometer. Since 1782 Julin observed himself with a mercurial thermometer made by Hasselstrom, in Stockholm. He took the trouble to compare the Florentine thermometer, degree by degree, with Hasselstrom's, and by that comparison corrected the whole of the preceding observations. But he still followed the method of his predecessor, and made observations only at six in the morning and six in the evening. Hence his mean temperature cannot be correct.

Mr. Tornsteen, in the Memoirs of the Swedish Academy for 1796, has given us a table of the curve of the daily temperature of every month from ten to ten days, as he had found it from ten years' observations at Brunslo, in Jämteland, in the 64th degree of latitude. The rate of the temperature differs but little from that at Uleoborg. But according to this table the yearly mean determined by observations at six in the morning and six in the evening is under the yearly mean determined by observing the extremes of the daily temperature 0·67° Reaumur or 1·51° Fahrenheit. This gives a considerable correction. The temperature at Uleoborg for every month, when this correction is attended to, will be as follows:*

	Julin's table.	Ditto corrected.	Amount of the correction.	Column 3d reduced to Fahrenheit's scale.
January	−11·84°R	−11·42°R	0·42°	6·31 ° F
February	−10·16	− 9·072	1·088	11·588
March	− 9·12	− 7·646	1·474	14·797
April	− 3·2	− 2·255	0·945	26·926
May	+ 2·8	+ 3·315	0·515	39·459
June	9·12	9·5	0·42	53·4
July	12·16	12·34	0·18	59·76
August	9·92	10·14	0·22	54·81
Sept.	4·06	5·5	0·94	44·4
October	0·96	2·056	1·096	36·626
November	− 5·92	− 5·416	0·504	19·814
December	− 9·94	− 9·14	0·3	11·44

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* I have inserted this table as in the original, to show the rate of the correction: but I have added in the last column the corrected mean temperature at Uleoborg reduced to the degrees of Fahrenheit's thermometer, for the sake of the English reader.—T.

If we compare the mean annual temperature at Uleoborg with that at Upsala, we shall find them to rise and fall together, as the following little table will show:—

Mean Yearly Temperature.

	At Uleoborg.	Therm.	At Upsala.	Therm.	Difference.
1776	−0·9°	Centigrade	+6·18°	Centigrade	7·08
1777	−2·2		4·25		6·45
1778	−2·5		4·89		7·39
1779	−0·9		7·36		8·26
1780	−3·4		4·70		8·1
1781	−3		5·98		8·98
Mean	−2·15		+5·54		7·686
1782	−0·1		4·444		4·5
1783	−2·5		5·7		8·2
1784	−1·9		3·54		5·44
1785	+0·5		3·84		3·3
1786	1		4·07		3·07
1787	1·9		5·15		3·25
Mean	0·183		+4·456		4·633

There is a striking discordancy between the differences in the first six and last six years of this table. But at the beginning of 1782 Mr. Julin laid aside the spirit of wine thermometer, and began to observe with Hasselstrom's mercurial thermometer. It would appear that notwithstanding his endeavours to correct the spirit of wine thermometer, it still stood lower than the mercurial one. On that account, if we reject the first six years' observations altogether, the mean temperature at Uleoborg will be as I have given it in page 344.

2. *On the Height at which different kinds of Trees grow.*

The hopes which I have held out in the preceding dissertation, that the difference in the height to which the different trees extend, which in Lapland is so constant, will be found to be equally so over the whole earth, are completely visionary. During my journey in Switzerland and Savoy in 1810, I observed quite a different state of things. The summer in Lapland and on the Alps is very dissimilar, and the monks of St. Bernard are in the right when they say, "The inhabitants of Lapland are fortunate, much more fortunate than we; they enjoy a warm summer favourable to life, while our summer is only a milder winter." Dr. Wahlenberg, in his *Flora Lapponica*, has placed this difference clearly before our eyes, by his exhibition of the curves representing the temperatures at *Enontekis* in Lapland, and at the cloister on *St. Gotthardt*. The effect which this difference must have upon the height to which trees vegetate, he has shown clearly by his journey of this summer in Switzerland. The *Scotch fir*, which in Lapland ascends far above the *spruce fir*, ceases to grow in Switzerland at the height of little more than 3000 feet, while the spruce fir rises higher than 7000 feet. The beech in Sweden is not to be found farther north than West Gothland; but upon the Alps it ascends to the climate of Lapland. The grey alder (*alnus incana*) stops considerably below the spruce fir, while in Lapland it is one of the last trees that yields to the severity of the climate.

In the valleys of the *Valais* and Savoy, as far as Mount Cenis, I have observed however a similar gradation in the height to which particular species of trees climb, if we attend only to particular localities. Such, for example, are the valleys, which being traversed as passes are quite naked of trees. The wind rushing through these passes does not permit trees to grow. We find trees much higher in valleys surrounded by snow mountains, or on the declivities at the heads of valleys. These declivities are so steep, that the warmth from the valley is able to reach them, and consequently to produce a modification in the height at which trees grow.

The following is a small table of the result of my observations between latitude 45¼° and 46½°, excluding the effect of such accidental causes :—

	English Feet.
Snow line	9080
Rododendron line	7290
Line of spruce firs	6842
Line of beeches	5132
Line of cherries	4438
Line of nuts	3798
Line of vines	2592

The difference between the absolute height of these lines in Savoy and in the northern parts of Switzerland is considerable. But the observations which I made in Appenzel are too uncertain and too crude to enable us to determine how far this difference holds in them all. At *Ammon*, above the lake of *Wallenstadt*, Mr. Horner and I observed the last nut-tree at the height of 3108 feet, the last cherry-tree at the height of 3556, and the last beech-tree at Thurgau at the height of 4458 feet. This gives us

	At Thurgau.	In the Valais.
The distance of the beeches from the nut-trees	1350 feet	1333 feet
Ditto between the beeches and cherries	902	694

If these differences be correct, the snow line in Appenzel must be 100 fathoms lower than in the Valais and in Savoy, and could not exceed the height of 8402 feet.

3. *Determination of the Heights of some Mountains and Passes in the country of the Grisons.*

In determining these heights I have employed a set of observations made by Mr. Escher in the Grisons, and I have reduced them according to the Lindenau tables. The mountains of the Grisons have been seldom measured, and the heights of the passes are almost unknown. Corresponding observations with those of Escher were made at Chur by Mr. Von Salis; and I have reckoned, with Lambert (*Acta Helvet.*), the height of Chur at 1695 English feet above the sea.

	Height above the sea.
Parpan, above *Churwalden*	4663 Eng. feet
Erosa, a side valley in the Schallsickthal	5848
Plessur, at the entrance of *Erosa* into it	4981
Furckli Scheideck, in Strela, the passage from Schallsickthal to the valley of Davos	7728
Cathedral of Davos	4845
Scaletta Scheideck, in the Engadin	8334
Zinnskel, in the upper Engadin, under Zutz	4996
Zernetz, in the lower Engadin, at the bridge	4541
Guarda, in the lower Engadin	5248
Fettan, in the lower Engadin	5005
Schils, 200 feet above the Inn	3800
Martinsbruck on the border	3190
Finstermung, in Tyrol	2993
Nauders	4165
*Reschen Scheideck**	4596
Mals	3309
Glurns in *Etschdal*	2756
St. Maria Munsterthal	4345
Ossen Scheideck, in the Munsterthal, by the *Engadin*, at *Zernetz*	691

ARTICLE IV.

Account of an Arithmetical Machine lately disc *Library of Edinburgh.* By W. A. Cadel

NICOLO TARTAGLIA, in his Treatise on in 1526, amongst other methods of per gives one which he illustrates by the follow product of 4567 by 326 is required :—

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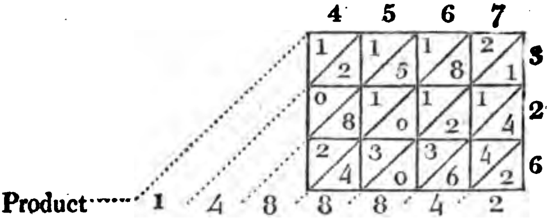
* This is probably the lowest pass through th ice mountain of *Oezthales*, are not far off. It the valley of the *Etsch*, and follows the centr

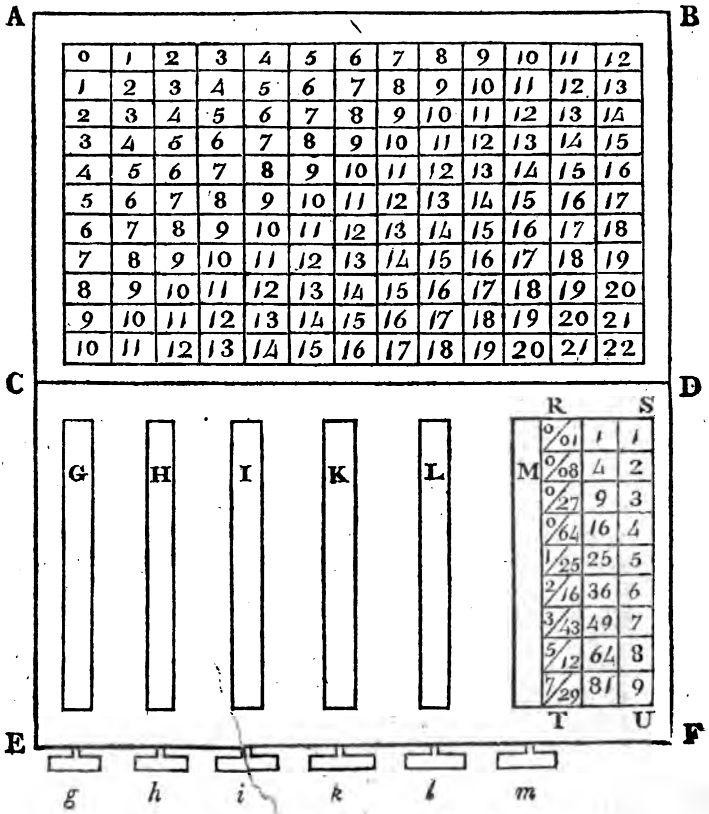
1



The great mathematician, Napier, afterwards employed this method in the construction of his virgulæ lamellæ, both of which are described in the Rabdologia published by him in 1617.

I lately met with a small arithmetical machine consisting of a disposition of Napier's rods, which is not described in his Rabdologia. This system is contained in a small box of oak. It was found amongst some old books in the library of the University of Edinburgh, and was probably made after the time of Napier.

The annexed diagram, which is about two-thirds the size of the original, represents the box when open; the hinge is on the line C D, and A, B, C, D, is the lid; the table inscribed

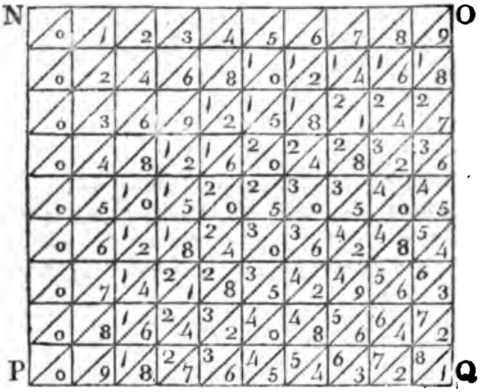


0	1	2	3	4	5	6	7	8	9	10	11	12
1	2	3	4	5	6	7	8	9	10	11	12	13
2	3	4	5	6	7	8	9	10	11	12	13	14
3	4	5	6	7	8	9	10	11	12	13	14	15
4	5	6	7	8	9	10	11	12	13	14	15	16
5	6	7	8	9	10	11	12	13	14	15	16	17
6	7	8	9	10	11	12	13	14	15	16	17	18
7	8	9	10	11	12	13	14	15	16	17	18	19
8	9	10	11	12	13	14	15	16	17	18	19	20
9	10	11	12	13	14	15	16	17	18	19	20	21
10	11	12	13	14	15	16	17	18	19	20	21	22

0/01	1	1
0/08	4	2
0/27	9	3
0/64	16	4
1/25	25	5
2/16	36	6
3/43	49	7
5/12	64	8
7/29	81	9

on the lid is a table of addition, the figure in the intersection of a line and a column, being equal to the figure at the head of the column, plus the figure at the left extremity of the line.

C, D, E, F, is the body of the box; G, H, I, K, L, M, are six slits, under each of which, in the thickness of the box, which is about half an inch, a cylinder is placed, which may be turned round its axis by a handle, *g*, *h*, *i*, *k*, *l*, *m*; on each of these cylinders is pasted the table N, O, P, Q, which is a multiplication table



0	1	2	3	4	5	6	7	8	9
0	2	4	6	8	1/0	1/2	1/4	1/6	1/8
0	3	6	9	1/2	1/5	1/8	2/1	2/4	2/7
0	4	8	1/2	1/6	2/0	2/4	2/8	3/2	3/6
0	5	1/0	1/5	2/0	2/5	3/0	3/5	4/0	4/5
0	6	1/2	1/8	2/4	3/0	3/6	4/2	4/8	5/4
0	7	1/4	2/1	2/8	3/5	4/2	4/9	5/6	6/3
0	8	1/6	2/4	3/2	4/0	4/8	5/6	6/4	7/2
0	9	1/8	2/7	3/6	4/5	5/4	6/3	7/2	8/1

disposed in the manner of Napier's rods, the line N P being parallel to the axis of the cylinder V to the slit. If the solution of the above example from Tartaglia be required, turn the handle *i* till the column which has 4 at top appears through the slit, turn *k* till the column 5 appears, *l* till 6, and *m* till 7 is seen, then write out the line 3 of these four columns, write out the line 2, and the line 6, and we have the figures disposed as in the example: upon adding them diagonally, the product is obtained.

R, S, T, U, is the lamina for the extraction of the cube root, as described in the Rabdologia.

ARTICLE V.

Essay on the Cause of Chemical Proportions, and on some Circumstances relating to them: together with a short and easy Method of expressing them. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

(*Concluded from* p. 255.)

14. *Platinum* (Pt).—I have already in a preceding memoir given an account of my experiments to determine the capacity of saturation of this metal. Several metals, particularly rhodium, platinum,

gold, mercury, and copper, have two salifiable oxides, the protoxide of which in all these metals has a striking analogy. These protoxides, which I distinguish by the Latin termination *ossum*, differ in so striking a degree from the protoxides of iron, manganese, and cerium, that there must be some general difference in their composition. The peroxides of the first named metals have more characteristic marks of salifiable bases than the protoxides; the contrary is the case with the oxides of iron, manganese, and cerium. The peroxides of the first class contain twice as much oxygen as the protoxides, while in the three other metals the peroxides contain only 1½ times as much oxygen as the protoxides. It appears from this that the peroxides of platinum, gold, copper, mercury, are proportional in their composition to the protoxides of iron, manganese, and cerium; that is to say, that both contain 2 volumes of oxygen. This circumstance, added to the fact that the protoxide of gold cannot possibly contain more than 1 volume of oxygen, has induced me to conclude that the protoxides of rhodium, platinum, gold, mercury, and copper, are composed of equal volumes of radicle and oxygen. In consequence of all this, if, according to my experiments, the protoxide of platinum is composed of 100 metal and 8·287 of oxygen, the volume of platinum must weigh 1206·7.

15. *Aurum, gold* (Au).—A hundred parts of gold by my experiments combine in the peroxide with 12·077 of oxygen; in the protoxide, with $\frac{1}{3}$ of that quantity. If this protoxide be Au + O, as is probable, then the volume of gold weighs 2483·8. As the peroxide of gold is Au + 3 O, it follows that a deutoxide Au + 2 O must exist. I have endeavoured to prove that the purple of Cassius contains this deutoxide, and that the formation of the powder is inexplicable without the existence of this intermediate oxide. M. Proust conceives that the purple of Cassius is a combination of metallic gold with the oxide of tin. But such a combination is contrary to every known analogy respecting the circumstances under which the metals combine with other oxides. But the solubility of the purple of Cassius in ammonia, which Proust discovered, and which I have verified, proves sufficiently that the gold in it is in the state of an oxide. Now the purple of Cassius cannot contain protoxide of gold, because that oxide forms green or yellow combinations; neither can it contain the peroxide, because it is formed by the reduction of that oxide to a lower degree of oxidation. It appears, then, clearly to follow, that it must contain a deutoxide, which, like the deutoxide of rhodium, is not salifiable, though it be capable of combining with the oxide of tin; and by means of this last, likewise, with ammonia. It seems to be the deutoxide of gold that gives the purple colour to animal and vegetable matter treated with muriate of gold.

16. *Palladium* (Pa).—The analogy between platinum and palladium would lead one to expect that this last forms two oxides as well as the first. But I have only been able to discover a single

oxide of this metal, and its soluble muriate is decomposed without producing any other muriate. If we burn palladium filings in a platinum crucible along with caustic potash and a little saltpetre, the palladium is oxidated. We obtain a chesnut coloured oxide, which contains potash, but which dissolves in muriatic acid without the disengagement of oxymuriatic gas, and forms the common muriate. I have found that 100 of palladium combine with 14·209 to form the oxide of palladium. The oxide thus formed bears all the characters of containing more than 1 volume of oxygen. If we suppose it to contain 2, the volume of palladium will weigh 1407·56.

17. *Argentum, silver* (Ag).—I have determined the composition of the oxide of silver from the sulphuret, because it was not possible to have so exact a result from the direct analysis of the oxide. I found by this means that 100 silver combine with 7.44 of oxygen. But as we do not know the volume of sulphur with precision, it is possible that the oxygen may amount only to 7·3575. The oxide of silver appears to contain 2 volumes of oxygen. Hence the volume of metal will weigh 2688·17 or 2718·31.

18. *Hydrargyrum, mercury* (Hy).—Mr. Sefstrom has examined with care the composition of the oxides and sulphurets of this metal. According to his experiments, 100 of mercury combine to form the red oxide with 7·89, 7·9, or 7·99, of oxygen, and with half as much to form the black oxide. As this last belongs to those oxides which I conceive formed of equal volumes of oxygen and metal, the volume of mercury ought to weigh 2531·6. Its minimum should be 2503·13, and its maximum 2536·1.

19. *Cuprum, copper* (Cu).—I have found that 100 copper combine with 24·8 or 25 oxygen to become black oxide, and with half as much to form the protoxide. If this last be $Cu + O$, the volume of copper ought to weigh 806 48, or at a minimum 800.

20. *Niccolum, nickel* (Ni).—Messrs. Rolhoff and Tupputi have examined the oxides of this metal, with results agreeing well with each other. Rolhoff found that a solution of neutral muriate of nickel, which contained 1·88 of oxide of nickel, gave with nitrate of silver 7·182 of fused muriate of silver; that is to say, that 100 muriatic acid neutralize 137·52 of oxide of nickel; from which it follows that the oxide is composed of 100 metal and 27·255 of oxygen. Some experiments of Rolhoff induced him to believe that the peroxide of this metal contains $1\frac{1}{3}$ or $1\frac{1}{2}$ as much oxygen as the protoxide. In the first case the protoxide would be $Ni + 3\,O$; in the second case, $Ni + 2\,O$. I consider the last as most probable. Hence the volume of metal should weigh 733·8.

Bucholz has rendered it probable that nickel has an oxide containing less oxygen than either of the preceding. We obtain it when we decompose by means of caustic potash the yellow sublimate which is obtained when muriate of nickel is distilled in a retort. The existence of this oxide deserves a more particular examination.

21. *Cobaltum, cobalt* (Co).—By analogous experiments to those made with nickel, Mr. Rolhoff found that 100 parts of muriatic acid are neutralized by 137·345 of the oxide of cobalt. Hence it follows that this metal combines with 27·3 of oxygen. Mr. Rolhoff found likewise that 100 parts of peroxide of cobalt exposed to heat lose from 9·5 to 9·9 of oxygen, and are reduced to the state of protoxide. Hence the peroxide contains 1½ times as much oxygen as the protoxide. Hence the protoxide is Co + 2 O, and the volume of cobalt weighs 732·61.

It is a very remarkable circumstance that these two metals, nickel and cobalt, which so frequently accompany each other in nature, have, not only in their gaseous state, but likewise when in a solid form, an equal specific gravity.

22. *Bismuthum, bismuth* (Bi).—Mr. Lagerhjelm has found that this metal combines with 11·275 of oxygen for the 100 of bismuth. I have found that this metal when exposed to the air forms a purple coloured protoxide. Hence the known oxide must be at least Bi + 2 O. In that case the volume of bismuth will weigh 1774.

The analogy between antimony and bismuth led me to expect that it would be converted into an acid when heated with nitrate of potash; but I obtained only the common oxide, which even when in a state of fusion did not mix with the fused nitre.

23. *Plumbum, lead* (Pb).—I have found that 100 parts of this metal combine in its three known oxides with 7·7, 11·55, and 15·4 of oxygen. But these numbers are to each other as 2, 3, 4. I have endeavoured to prove that the tarnished and blackish surface, which lead acquires when exposed to the moist air, is a protoxide of that metal. Hence it follows that the yellow oxide must be Pb + O. The volume of lead then weighs 2597·4.

24. *Stannum, tin* (Sn).—I have proved in another memoir that tin has three degrees of oxidation, and that the protoxide is composed of 100 metal and 13·6 oxygen, while the peroxide contains twice that quantity. As these three oxides are all salifiable, it is probable that their respective quantities of oxygen are to each other as the numbers 2, 3, 4, and not as the numbers 3, 4, 6, because this last progression belongs to the oxides which have acid properties, as those of antimony, arsenic, and chromium. Besides, the progression of oxidation of a metal ought to be the same as that of its different degrees of sulphuration; and I have proved that tin has three sulphurets, in which the sulphur is as the numbers 2, 3, 4, and the two extremes of which are proportional to the protoxide and peroxide of tin. Hence the intermediate sulphuret and oxide must have a proportional composition. From these observations it follows that the three oxides of tin are Sn + 2 O, Sn + 3 O, Sn + 4 O, and that the volume of this metal weighs 1470·49.

25. *Ferrum, iron* (Fe).—I have found that the purest hammered iron contains always about half a per cent. of carbon, and that it produces 143·5 of red oxide, which of course contains only 99·5 of iron: 100 of iron, then, combine with 44·25 of oxygen. The

care with which I made these experiments, and the agreement between them, induce me to believe that they approach as near accuracy as a direct experiment can do. Other chemists have since published analyses of this oxide. Gay-Lussac, for example, found that 100 of iron combined with 42·35 of oxygen; but he seems to have paid no attention to the carbon present, nor to my experiments, though they had been published in Paris several months before his. (Ann. de Chim. Nov. 1811, p. 163.) The confidence to which every thing is entitled that bears the name of this celebrated chemist, may perhaps throw some doubts respecting the accuracy of my analysis. However, till some other chemist has verified our results, I hope I may be excused, in consequence of the great care which I bestowed in order to obtain exact results, if I consider my own numbers as approaching most nearly to the truth.

By other experiments I have shown that the oxygen in the black oxide is to that in the peroxide as 2 to 3. Hence it follows that the two oxides of iron are $Fe + 2\ O$, $Fe + 3\ O$. The volume of iron then weighs 693·64.

M. Gay-Lussac has discovered that the black oxide, formed when iron is exposed at a high temperature to the vapour of water, contains more oxygen than the common black oxide of iron. According to the numbers given by Gay-Lussac, iron in that oxide is combined with $1\frac{1}{4}$ as much oxygen as in the common black oxide. He considers it as a particular oxide capable of becoming the basis of salts. Yet as its sulphate is decomposed by alcohol in such a manner that the persulphate of iron is dissolved and the green sulphate left untouched, as the succinates and benzoates precipitate from it persuccinate and perbenzoate of iron, and as the caustic alkalies precipitate from it persubsulphates of iron, before precipitating the green sulphate, it is clear that this substance cannot be considered as a particular oxide, but as a combination of the black and peroxides of iron. M. Proust has long since proved that common prussiate of potash is a triple prussiate of potash and black oxide of iron. When we precipitate metallic solutions by this prussiate, the potash is exchanged for the metallic oxide, which constitutes with the black oxide of iron a triple prussiate. When the potash is exchanged for peroxide of iron we have as usual a triple prussiate containing the two oxides of iron for its bases: and it is clear that in such a case the peroxide of iron exists in the salt just in the same state that any other metallic oxide would. Hence it follows that triple salts containing these two oxides as bases really exist.

Gay-Lussac pretends that the Swedish minerals attracted by the magnet contains his new oxide. I have examined several of these minerals without ever finding any proofs of his assertion. I have pulverized these minerals, and have extracted the part attracted by the magnet under water, in order to separate it from the matrix and the red oxide of iron mechanically mixed. When I treated what the magnet had attracted with less diluted muriatic acid than was sufficient to dissolve the whole, it has frequently happened to me that the whole black oxide was dissolved and a beautiful red oxide left behind. When I treated the mass with nitromuriatic acid, I oxidated the black oxide, and then precipitated the whole with caustic ammonia. The red oxide thus obtained being well washed and dried, had gained an increase of weight by the oxygen absorbed by the black oxide. But in all my experiments this increase was so little that the black oxide must have been combined with a quantity of red oxide 3, 5, or 6 times that of the black; that is to say, that these minerals contain much more peroxide than the oxide produced by the vapour of water does.

Several chemists are of opinion that there is a white oxide of iron. I made the following experiment to verify this opinion. In a phial I poured diluted muriatic acid on pure iron filings, and I boiled them together till the acid was saturated. During this operation the phial was closed by a tube which conveyed the hydrogen gas under water. I had placed a phial of caustic potash quite in the neighbourhood of this mixture, and I had boiled it half an hour to drive off the atmospheric air. When the muriatic acid was saturated I decanted the muriate by means of a funnel which went to the bottom of the alkaline ley, and continued pouring it in till the phial was completely filled. I then stopped its mouth with a tube, which passed into a small pneumatic apparatus. At the line of contact of the two liquids in the phial a white precipitate appeared, and when I mixed the liquids the whole became thick and white. I then heated the whole till it boiled. The liquid increasing in bulk made its escape through the tube, and the white matter at the bottom of the phial became black, and the same change gradually took place in the whole precipitate, without the disengagement of any gas whatever to prove that it had absorbed oxygen during this change; for in that case it would have decomposed water, and hydrogen of course would have been disengaged. This experiment proves that the pretended white oxide is only the hydrate of the black oxide, which, like the hydrates of tin and copper, is decomposed at the temperature of boiling water. Hence it follows that the protoxide of iron is a black matter, which sometimes owes its white colour to water, sometimes to carbonic acid, as in sparry iron.

I consider these experiments as proving that there is no probability that there exists an intermediate oxide between the black and red oxides of iron.

26. *Zincum, zinc* (Zn).—I have found that 100 parts of zinc combine with 24·8 of oxygen to form the oxide of zinc, a result which Gay-Lussac has also drawn from his experiments. As zinc has a suboxide, and as the known oxide of this metal is analogous to those which contain more than one volume of oxygen, we may suppose that it is $\text{Zn} + 2\,\text{O}$, and in that case the volume of zinc will weigh 806·48, as is the case with copper and tellurium.

27. *Manganesium, manganese* (Mn).—It is proved by the expe-

riments of Dr. John, as well as by some experiments which I have made, that manganese has at least four degrees of oxidation, in the proportion of 1, 2, 3, 4: and as the tritoxide, or $Mn + 3\ O$, is composed of 100 metal and 42·16 oxygen, it follows that the weight of a volume of manganese is 711·575.

28. *Cerium* (Ce).—Mr. Hisinger has found that 100 of muriatic acid are neutralized by 196·18 of protoxide of cerium, and that the percarbonate is composed of 63·83 oxide and 36·17 of carbonic acid. According to these experiments, 100 cerium form protoxide by uniting with 17·41 of oxygen, and peroxide by uniting with 26·115; that is to say, with 1½ as much as in the protoxide. Hence these oxides ought to be $Ce + 2\ O$, $Ce + 3\ O$. Therefore a volume of cerium must weigh 1148·8.

29. *Uranium* (U).—Unknown.

30. *Yttrium* (Y).—I decomposed dry carbonate of yttria in a small retort furnished with a tubulated receiver, into the tube of which I had put muriate of lime. When the retort began to soften in the fire, I allowed it to cool, and took out the earth, which I exposed to a still stronger heat in a platinum crucible. 100 parts of carbonate thus treated gave 12·82 of water and 57·7 of yttria. Hence it is composed of

Yttria	57·70
Carbonic acid	29·48
Water	12·82
	100·00

The 29·48 of carbonic acid contain 21·446 of oxygen; and as the neutral muriate of yttria is precipitated by the carbonate of ammonia without disengagement of carbonic acid, and as in this carbonate the acid contains twice as much oxygen as the base, the same thing must hold likewise in the carbonate of yttria. Therefore 57·70 of yttria ought to contain 10·723 of oxygen: so that this earth contains 18·58 per cent. The 12·82 of water contain 11·3 of oxygen. We see from this circumstance that the carbonate had not been entirely deprived of its adherent water, but that in the carbonate the water and earth must contain equal quantities of oxygen.

2·8 parts of yttria deprived of carbonic acid were dissolved in pure sulphuric acid, the solution was evaporated to dryness, and the sulphate heated till it ceased to give out acid vapours. It now weighed 5·392 parts, and dissolved entirely in water. Sulphate of yttria, then, is composed of

Sulphuric acid	51·823	100
Yttria	48·177	108
	100·000	

Now if 108 parts of yttria contain 19·96 of oxygen, 100 parts ought to contain 18·49. This approaches very nearly to the result obtained by the preceding experiment.

As all the earths must be classed among oxides which contain more than one volume of oxygen, it is probable that they contain in general two volumes. In that case the volume of yttrium will weigh from 876·42 to 881·66.

I ought to observe that the yttria employed in these experiments contained no glucina, but I was not able to deprive it entirely of the manganese, a little of which is present in gadolinite.

31. *Glucinum* (Gl).—Unknown.

32. *Aluminum* (Al).—I have proved in another memoir that alumina contains 46·726 per cent. of oxygen. In alum the oxygen of the alumina is to that of the potash as 3 : 1. Hence it seems to follow that alumina contains three volumes of oxygen: but as the phenomenon is equally explicable on the supposition that alum contains three volumes of aluminum for one volume of potassium, the observation proves nothing with respect to the weight of a volume of aluminum. If we suppose alumina to be $Al + 2\,O$, the volume of aluminum will weigh 228; if we suppose it $Al + 3\,O$, the volume will weigh 342.

33. *Magnesium* (Ms).—Experiments on the composition of sulphate and muriate of magnesia indicate in that earth from 38·3 or 38·8 to 39·872 per cent. of oxygen. If we suppose magnesia to be $Ms + 2\,O$, the volume of magnesium will weigh 315·46; in its minimum 301·68, and in its maximum 321·93.

34. *Calcium* (Ca).—Lime contains, according to the analysis of the muriate and carbonate of lime, 28·169 per cent. of oxygen. Therefore if lime be $Ca + 2\,O$, the volume of calcium must weigh 510·2.

35. *Strontium* (Sr).—From the analysis of the muriate of this earth made by Sir H. Davy, 100 parts of muriatic acid are neutralized by 209 parts of strontian. Hence it follows that the earth contains 14·09 per cent. of oxygen. Therefore if strontian be $Sr + 2\,O$, the volume of strontium will weigh 1418·716.

36. *Barytium* (Ba).—From the analyses of the sulphate and muriate of barytes, the earth contains 10·47 per cent. of oxygen. Now, supposing barytes to be $Ba + 2\,O$, the volume of barytium will be 1709·1.

37. *Sodium* (So).—100 parts of sodium combine with 34·52 parts of oxygen, and the peroxide contains $1\frac{1}{2}$ times as much. Hence it follows that soda is $So + 2\,O$; and the volume of sodium must weigh 579·32.

38. *Potassium* (Po).—100 parts of this metal combine with 20·45 of oxygen in potash, and with three times as much in the peroxide. Hence potash should be $Po + 2\,O$, the peroxide $Po + 6\,O$, and the protoxide $Po + O$; and a volume of potassium should weigh 978.

In publishing this attempt to determine the specific weight of each elementary substance, supposing it in the state of gas, I ought not to conceal that I was fully aware of the impossibility of obtaining results which would not be liable, in consequence of subsequent experiments, to very considerable corrections and alterations. But I thought it better to prefer the advantage which the science would derive from such an attempt, imperfect as it may be, to the vain satisfaction of not being exposed to criticisms and corrections. I am satisfied that most of the determinations given in this memoir will be hereafter corrected in several particulars, without including those numbers which I have been in several cases obliged to adopt from analogies which may not in every case be correct.

I ought likewise to observe that in speaking of compound volumes, I had no intention of determining the real volumes of the substances: for example, in saying that sulphuric acid is S + 3 O, I do not mean to say that sulphuric acid when in the state of gas contains three volumes of oxygen and one volume of sulphur condensed into the bulk of one volume. On the contrary, the elementary volumes in combining undergo contractions in bulk, with the general laws of which we are not acquainted, though we know some examples. It will constitute a different, and probably much more difficult, study, to determine the specific gravity of each substance, supposing it in gas: though I think I perceive that we shall find means of calculating with considerable precision the contraction which the elementary volumes should undergo in combining. When I say, for example, that the subarseniate of lead is As $\overset{6}{O} + 1\frac{1}{2}\,P\overset{2}{O}$, the number $1\frac{1}{2}$ applies to the lead of which the compound contains $1\frac{1}{2}$ for each volume of arsenic. But it may happen that the result of calculation of the compound volumes will prove that the neutral arseniate is composed of one volume of acid and two volumes of oxide of lead; in which case the subarseniate ought to contain three volumes of the oxide.

Before concluding I ought to say a few words about a question very intimately connected with the present subject; namely, *What is the relation of the specific gravity of solid substances with their specific gravity when in the state of gas?* In casting our eyes over the comparative table of these weights at the end of this memoir, we see too great a discordance between them to draw the consequence that there is a relation between them. On the other hand, we sometimes meet with coincidences which ought not to be neglected; because it is possible that when the gaseous volumes are rectified, and the weights of the solid substances more accurately determined, by using only bodies perfectly pure: for example, by weighing the metals only in the state of greatest purity that can be obtained; for hitherto they have been more or less contaminated with carburet; and by reducing all the results to the same temperature, it is possible that we may hereafter perceive coincidences which escape us at present. When we compare in the following table the weights of sulphur, phosphorus, and arsenic, we find that the first, both solid and gaseous, has a weight very nearly approach-

ing to 2; phosphorus in a solid state weig
and arsenic in a solid state 8·31, in a gas
pointed out the correspondence betwee
between tellurium and antimony. On
copper, when in the gaseous form, have
differ considerably in their solid state. P
in the gaseous state only by half the w
the solid state it is 29 times heavier.

There is another point which deserve
the relation between the specific gravi
the contraction which its elements un
no doubt that by such an examinatio
mine not only the specific gravity f
the specific gravity of compound vol
is to say, we should be able in that v
which their elements have underg
searches it is probable will have con
lopement of the theory of atoms.

Comparative Table of the Specific

Names.	Symbols.	Weig form
Oxygen	O	1(
Sulphur	S	2(
Phosphorus	P	1
Muriatic radicle	M	1
Fluoric radicle	F	
Boron	B	
Carbon	C	
Nitric radicle	N	
Hydrogen	H	
Arsenic	As	
Molybdenum	Mo	
Chromium	Ch	
Tungsten	Tn	
Antimony	Sb	
Tellurium	Te	
Columbium	Cl	
Titanium	Ti	
Zirconium	Zr	
Silicium	Si	
Osmium	Os	
Iridium	I	
Rhodium	Rl	
Platinum	P	
Gold	A	
Palladium	P	
Silver	A	
Mercury		
Copper		
Nickel		
Cobalt		
Bismuth		
Lead		

Names.	Symbols.	Weight in form of gas.	Ditto at a minimum.	Ditto at a maximum.	Sp. gr. in a solid form.
Tin	Sn	1470·59	....	....	7·299
Iron	Fe	693·64	....	....	7·788
Zinc	Zn	806·45	....	....	7·215
Manganese	Ma	711·575	....	....	8·013
Uranium	U	....	....	....	....
Cerium	Ce	1148·8	....	....	....
Yttrium	Y	881·66	876·42	....	....
Glucinum	Gl	....	....	....	....
Aluminum	Al	228·025	....	342·	....
Magnesium	Ms	315·46	301·63	321·93	....
Strontium	Sr	1418·14	....	....	....
Barytium	Ba	1709·1	....	....	....
Calcium	Ca	510·2	....	....	....
Sodium	So	579·32	....	....	0·9348
Potassium	Po	978·0	....	....	0·8

Explanation of some Compound Chemical Signs.

$S + 2 O$. $S + 3 O$.—Sulphurous acid, sulphuric acid.

$M + 2 O$. $M + 3 O$.—Muriatic acid, oxymuriatic acid.

$2 H + O$. $6 H + N + O$.—Water, ammonia.

$N + O$. $N + 3 O$.—Nitrous oxide, nitrous gas.

$As + 3 O$. $As + 6 O$.—Arsenic oxide, arsenic acid.

$Fe + 2 O$. $Fe + 3 O$.—Black oxide of iron, red oxide of iron.

$Po + 2 O$. $Po + 6 O$.—Potash, peroxide of potassium.

$2 H + S$. $As + 12 S$.—Sulphureted hydrogen, supersulphuret of arsenic.

$Sb + 3 S$. $St + 2 S$.—Sulphuret of antimony, sulphuret of tin.

$2 N \overset{6}{O} + Po \overset{2}{O}$.—Nitrate of potash.

$2 S \overset{3}{O} + Po \overset{2}{O}$.—Sulphate of potash.

$S \overset{3}{O} + Cu O$.—Sulphate of copper.

$As \overset{6}{O} + Cu \overset{2}{O}$.—Perarseniate of copper.

$M \overset{2}{O} + \overset{6}{H} N O + \overset{2}{H} O$.—Hydrous muriate of ammonia.

$2 Te \overset{2}{O} + Pb \overset{2}{O}$.—Telluriate of lead.

$3 M \overset{2}{O} + Sb \overset{3}{O}$.—Muriate of antimony.

$N \overset{6}{O} + 3 Pb \overset{2}{O}$.—Subnitrate of lead at a minimum.

$M \overset{2}{O} + 2 Cu \overset{2}{O}$.—Persubmuriate of copper.

$\mathrm{As}\,\overset{6}{\mathrm{O}} + 1\tfrac{1}{2}\,\mathrm{Pb}\,\overset{2}{\mathrm{O}}$.—Subarseniate of lead.

$\mathrm{S}\,\overset{3}{\mathrm{O}} + 1\tfrac{1}{2}\,\mathrm{Cu}\,\overset{2}{\mathrm{O}}$.—Persubsulphate of copper.

$2\,\mathrm{S}\,\overset{3}{\mathrm{O}} + \mathrm{So}\,\overset{2}{\mathrm{O}} + 20\,\overset{2}{\mathrm{H}}\,\mathrm{O}$.—Crystallized sulphate of soda.

$2\,\mathrm{S}\,\overset{3}{\mathrm{O}} + \mathrm{Zn}\,\overset{2}{\mathrm{O}} + 10\,\overset{2}{\mathrm{H}}\,\mathrm{O}$.—Hydrous sulphate of zinc.

$\mathrm{S}\,\overset{3}{\mathrm{O}} + \overset{6}{\mathrm{H}}\,\mathrm{N}\,\mathrm{O} + 2\,\overset{2}{\mathrm{H}}\,\mathrm{O}$.—Hydrous sulphate of ammonia.

$2\,\mathrm{S}\,\overset{3}{\mathrm{O}} + \mathrm{Cu}\,\overset{2}{\mathrm{O}} + 4\,\overset{6}{\mathrm{H}}\,\mathrm{N}\,\mathrm{O} + 2\,\overset{2}{\mathrm{H}}\,\mathrm{O}$.—Hydrous ammonio-subsulphate of copper.

Article VI.

On the Composition of Azote. By John Miers, Esq.

(To Dr. Thomson.)

SIR,

It is now about two years since I was directed, from some unaccountable phenomena, to the consideration of the nature of azote, and was induced to institute a series of experiments, with a view of effecting either its composition or its decomposition. The results exceeded my most sanguine expectations, and I succeeded in obtaining, from the decomposition of water, gases possessing all the negative properties of azote. In order to denote minutely the absolute changes that took place, and to ascertain the proportions of the constituents with more accuracy, I determined to institute a new series of experiments, with more perfect and improved apparatus; and with this view my labours were suspended till I could get all completed. Unforeseen and unavoidable circumstances, however, sprung up in the interim, which continued to delay the resumption of my exertions until the last summer, when, on the point of making arrangements for the consummation of my intentions, a notice appeared in the first volume of your *Annals*, p. 466, stating that Berzelius, by calculation, had ascertained azote to be "a compound of 44·6 unknown inflammable gas and 55·4 oxygen gas." The near coincidence of this determination with the results of my own calculations could not but excite my attention, and no time was lost in communicating to you an annunciation, as noticed in page 63 of the succeeding volume. I then, it is true, promised to transmit you an account of my labours, as soon as I could collect a series of experiments sufficiently satisfactory to convince the chemical world: this promise would long ere this have been performed, had not the discovery of a *new acid gas*, formed in the residue of my former experiments, possessing very singular properties, arrested my progress, and absorbed the whole of the small degree of time that my professional avocations could allow me to devote to these interesting pursuits.

Your valuable paper in N° XIV. p. 134, of the *Annals*, could not but raise my attention; and as the pleasing gratification of seeing removed your objections to the compound nature of azote, and as the determinations at which you have now arrived correspond so exactly with those I had long since deduced, I thought it proper, as you have so justly called upon me to fulfil my promise, to delay no longer transmitting you the annexed table, with the subsequent observations. The singular coincidence of the various proportions, all agreeing so closely with your recent determinations, with those of Berzelius, and with the results of experiment, cannot fail to excite the attention of chemists; and if the facts which I hope shortly to make known to you should be considered decisive, I shall feel proud that my humble exertions can have in any way contributed to the advancement of the immortal work in which you are so pre-eminently engaged. The mere table by itself is a very strong presumptive evidence of the composition of azote, and must strike the most casual observer of the exact coincidence of calculation with experiment; but if, still farther, it be placed beyond doubt by the decisive test of fact, electro-chemical science will receive a new light, and the atomic theory will gain a fresh accession of strength, as the cause of the infinitely numberless chemical combinations will be more readily conceived when we perceive so many various compounds formed of two kinds of simple atoms only.

An atom of	Is composed of atoms of		Or of atoms of		In the proportion of		Proportions by Weight in 100 Parts.								Proportions			Weight of atom calculated.	
							Calculated from an atom of hydrogen.		According to experiment.		Calculated from an atom of azote.		According to experiment.		As oxides of nitric calculated by Berzelius.			From that of hydrogen.	From that of azote.
	Azote	Oxygen	Hydrogen	Oxygen	Hydrogen	Oxygen	Hydrogen	Oxygen	Hydrogen	Oxygen	Azote	Oxygen	Azote	Oxygen	Nitric	Hydrogen	Oxygen	0·13314	1·798
Water.......	..	..	1	1	1	1	11·75	88·25	11·75	88·25	..	..	..	..	..	..	..	1·133	..
Nitric acid...	1	5	6	6	1	1	11·75	88·25	..	..	26·46	73·54	26·43	73·57	11·71	..	88·29	6·798	6·798
Nitrous acid .	1	3	6	4	1½	1	16·64	83·36	..	..	37·47	62·53	37·41	62·59	16·55	..	83·45	4·798	4·798
Nitrous gas..	1	2	6	3	2	1	21·029	78·971	..	..	47·362	52·638	47·38	52·62		..		3·798	3·798
Nitrous oxide	1	1	6	2	3	1	28·54	71·46	..	..	64·3	35·7	64·3	35·7		..		2·798	2·798
Azote	1	0	6	1	6	1	44·409	55·591	..	..	100·	..	..	..	44·32	..	55·68	1·798	1·798
	Azote	Hydrogen									Azote	Hydrogen	Azote	Hydrogen					
Ammonia....	1	3	9	1	9	1	54·51	45·49	..	..	81·95	18·05	82	18			46·88	2·197	2·197
Ammonium ..	1	12	18	1	18	1	70·55	29·45	..	..	52·97	47·03						3·395	3·395

1. *Water.*—An exact knowledge of the composition of water affords the easiest and most correct data for ascertaining the weight of an atom of hydrogen, and as it seems generally agreed to be composed of 88·25 oxygen and 11·75 hydrogen, we can immediately arrive at its estimation by comparing the weights of the constituents with each other; hence the weight of an atom of hydrogen is to one of oxygen as 0·13314 to 1. Although this number differs a trifle from that fixed upon by you in page 42 of the second volume of your *Annals*, yet the result above given seems the natural result of the proportions just cited. This number I have therefore adopted in the foregoing calculations.

2. *Nitric Acid.*—That nitric acid should consist of the same proportions of oxygen and hydrogen as water, at first view startles us; but the surprise will immediately vanish on more minute examination. Although both consist of the same proportions of oxygen and hydrogen, yet the elementary atoms are arranged in different order. Each atom of water is simply composed of two elementary atoms, one of oxygen and one of hydrogen; while every atom of nitric acid consists of twelve elementary atoms, six of oxygen and six of hydrogen, or of one compound atom of azote and five elementary atoms of oxygen: their arrangement will be six atoms of hydrogen round one of oxygen, forming an atom of the second degree, and five atoms of oxygen again surrounding these, the whole forming an atom of the third degree, or of three series. This view of the subject enables us to conceive more clear ideas of the nature of affinity; as it will account for the small force with which the oxygen is held in it, and will exhibit sufficient cause why it is always resolved into oxides of azote of a lower degree, or into azote and oxygen, and never into hydrogen and oxygen separately. The proportion of 1 A + 5 O, which I had fixed upon as the probable constitution of nitric acid, is precisely the same as that determined by you in the last number of your *Annals*. It is equal to 6 H + 6 O.

By thus considering nitric acid to be formed of an equal number of atoms of oxygen and hydrogen, we arrive at its probable constitution as composed of azote and oxygen. Thus as $\overset{H}{44{\cdot}4} : \overset{O}{55{\cdot}6} :: \overset{H}{11{\cdot}75} : \overset{O}{14{\cdot}71}$. Then $\overset{H}{11{\cdot}75} + \overset{O}{14{\cdot}71} = \overset{Az}{26{\cdot}46}$, and $\overset{O}{88{\cdot}25} - \overset{O}{14{\cdot}71} = \overset{O}{73{\cdot}54}$. Hence 100 parts of nitric acid are composed of 26·46 azote and 73·54 oxygen. Now the proportion of azote to oxygen in nitric acid ascertained by the experiments of Berzelius (*Annals*, ii. 283,) is 26·43 azote to 73·57 oxygen—quantities singularly correspondent with those just determined. The calculations of Berzelius, who considers nitric acid to be composed of 11·71 nitric and 88·29 oxygen, correspond also equally well with those I have deduced for the proportions of oxygen and hydrogen, and

An atom of	Is composed of atoms of		Or of atoms of		In the proportion of		Proportions by Weight in 100 Parts. Calculated from an atom of hydrogen.		According to experiment.		Calculated from an atom of azote.		According to experiment.		Proportions As oxides of nitric calculated by Berzelius.			Weight of atom calculated. From that of hydrogen.	From that of azote.
	Azote	Oxygen	Hydrogen	Oxygen	Hydrogen	Oxygen	Hydrogen	Oxygen	Hydrogen	Oxygen	Azote	Oxygen	Azote	Oxygen	Nitric	Hydrogen	Oxygen	0·13314	1·798
Water.......	..	..	1	1	1	1	11·75	88·25	11·75	88·25	..	..	..	..	..	..	..	1·133	..
Nitric acid...	1	5	6	6	1	1	11·75	88·25	..	..	26·46	73·54	26·43	73·57	11·71	..	88·29	6·798	6·798
Nitrous acid .	1	3	6	4	1½	1	16·64	83·36	..	..	37·47	62·53	37·41	62·59	16·55	..	83·45	4·798	4·798
Nitrous gas..	1	2	6	3	2	1	21·029	78·971	..	..	47·362	52·638	47·38	52·62		..		3·798	3·798
Nitrous oxide	1	1	6	2	3	1	28·54	71·46	..	..	64·3	35·7	64·3	35·7		..		2·798	2·798
Azote	1	0	6	1	6	1	44·409	55·591	..	..	100·	..	..	..	44·32	..	55·68	1·798	1·798
	Azote	Hydrogen									Azote	Hydrogen	Azote	Hydrogen					
Ammonia....	1	3	9	1	9	1	54·51	45·49	..	..	81·95	18·05	82	18			46·88	2·197	2·197
Ammonium ..	1	12	18	1	18	1	70·55	29·45	..	..	52·97	47·03						3·395	3·395

Hence as $\overset{H}{44{\cdot}4} : \overset{O}{55{\cdot}6} :: \overset{H}{28{\cdot}54} : \overset{O}{35{\cdot}76}$; and $\overset{H}{28{\cdot}54} + \overset{O}{35{\cdot}76} = \overset{Az}{64{\cdot}3}$; and $\overset{O}{71{\cdot}46} - \overset{O}{35{\cdot}76} = \overset{O}{35{\cdot}7}$. Hence 100 parts of nitrous oxide are constituted of 64·3 azote and 35·7 oxygen. Now as nitrous oxide contains only half as much oxygen as nitrous gas for an equal volume of azote, it follows that as $\overset{Az}{60{\cdot}656} : \overset{O}{33{\cdot}672} :: \overset{Az}{64{\cdot}3} : \overset{O}{35{\cdot}7}$, proportions exactly corresponding to those just given. The determinations of nitrous gas and nitrous oxides were the only two instances where the results of experiment did not coincide with those of calculation; but these discrepancies have now disappeared since you have determined that nitrous gas is not composed of equal bulks of azote and oxygen, but of the volumes stated above.

6. *Azote.*—I was led to the determination of 6 atoms of azote + 1 atom of oxygen for an atom of azote by the results of my experiments, and on application to the calculation of the nitric compounds it was found to agree admirably with the proportions in which chemists had ascertained them to exist. The constitution of azote was thus deduced: $\overset{H}{11{\cdot}75} \times 6 + \overset{O}{88{\cdot}25} = \overset{H}{70{\cdot}5} + \overset{O}{88{\cdot}25} = \overset{H}{44{\cdot}409} + \overset{O}{55{\cdot}591} = \overset{Az}{100}$. Now Berzelius, by the most singular and skilful calculation, deduces azote to be composed of 44·32 unknown inflammable basis and 55·68 oxygen. So curious a coincidence with the result just stated would leave but little doubt that this inflammable base is hydrogen. Whatever scruples may arise on the subject, I hope soon to be able to obviate. The weight of an atom of azote, then, will be $\overset{H}{0{\cdot}133} \times 6 + \overset{O}{1} = 1{\cdot}798$, a number differing only 0·005 from that determined by you in the last number of your *Annals*.

7. *Ammonia.*—It will be seen that my conclusions are at variance with yours in the weight of an atom of ammonia. You conceive it, as stated in your last, to be composed of 1 Az + 1 H, while I have determined it to be 1 Az + 3 H. The reason for my having fixed on this proportion is thus seen: as $\overset{Az}{1{\cdot}798} : \overset{H}{0{\cdot}133} \times 3 :: \overset{Az}{81{\cdot}95} : \overset{H}{18{\cdot}05}$—the proportions obtained in the analysis of ammonia, taking the means of the most accurate experiments of Davy, Henry, and Berthollet: this mean being 74·42 hydrogen and 25·58 azote in volume, we obtain for the proportional weights of 100 parts, 82 azote and 18 hydrogen. Hence I conceive it decisive that an atom of ammonia is 1 A + 3 H, or 9 H + 1 O. Then to determine its original elements, $\overset{H}{11{\cdot}75} \times 9 + \overset{O}{88{\cdot}25} = \overset{H}{105{\cdot}75}$

$+ \overset{O}{88\cdot25} = \overset{H}{54\cdot51} + \overset{O}{45\cdot49}$. Again: if $\overset{Az}{100} : \overset{Q}{55\cdot591} :: \overset{Az}{81\cdot95} : \overset{O}{45\cdot54}$, and $\overset{Az}{81\cdot95} - \overset{O}{45\cdot54} = \overset{H}{36\cdot41}$. Then $\overset{H}{18\cdot05} + \overset{H}{36\cdot41} = \overset{H}{54\cdot46}$. Then 100 Az = 54·46 H + 45·54 O. Now Berzelius, calculating from the quantity necessary to neutralize a certain portion of acid, inferred that ammonia must contain 46·88 per cent. of oxygen, which differs but little from that ascertained above. The arguments urged by this celebrated philosopher in support of his opinion that hydrogen and azote are oxides of one common base,* as well as his subsequent observations to prove that hydrogen contains no oxygen,† may be applied with great force in maintenance of the endeavour here attempted to show that hydrogen is the base of azote.

8. *Ammonium.*—Although the nature of this singular combination is much involved in obscurity, yet its constitution may in some degree be developed from the exposition of the composition of azote and of ammonia, and the opinion supported by the French chemists seems the most probable view of the subject. The direct experiments are not sufficiently conclusive to found any correct idea of its nature, but they would lead us to infer that it contains half as much oxygen as ammonium. If this be correct, an atom of ammonium will consist of 1 Az + 12 H, or 1 O + 18 H. Hence we may deduce its composition: $\overset{A}{1\cdot798} : \overset{H}{0\cdot133} \times 12 :: \overset{Az}{52\cdot97} : \overset{H}{47\cdot03}$. Again: $\overset{H}{11\cdot75} \times 18 + \overset{O}{88\cdot25} = \overset{H}{70\cdot55} + \overset{O}{29\cdot45}$. And as $\overset{O}{55\cdot6} : \overset{H}{44\cdot4} :: \overset{O}{29\cdot45} : \overset{H}{23\cdot517}$. And $\overset{O}{29\cdot45} + \overset{H}{23\cdot517} = \overset{Az}{52\cdot967}$. And $\overset{H}{70\cdot55} - \overset{H}{23\cdot517} = \overset{H}{47\cdot033}$: corresponding precisely with that just given. The weight of an atom of ammonium will then be $0\cdot133 \times 12 + 1\cdot798$, or $0\cdot133 \times 18 + 1 = 3\cdot394$.

We see here eight various compounds of oxygen and hydrogen; two of which only, water and azote, exist in a double series as simple combinations in atoms of a second order. All the others are compounds of a more complex kind: the example given of nitric acid will be sufficient to make known the disposition of their primary particles, which in like manner arrange themselves in three series or in atoms of a third order. But there are, besides these, other combinations of the same elements whose arrangements are still more complex, those of a fourth kind, or compounds of atoms of the second and third orders; among these are hydronitric acid, liquid ammonia, &c. There are again those of a fifth kind, or an union of atoms of the third order with each other, such as nitrate of ammonia, &c. It is needless to point out the several others that must occur to those who investigate the subject, as the whole vegetable and animal world present such numberless instances of wonderful arrangements of the most complex materials formed of a few primary elements by the most simple means that could have been devised. Should these views happen to be realized, how infinite is the extent to which their consideration would lead us. As it is seen that, by the union of the simple elements with two kinds of compound atoms of a double series, the one formed of a particle of oxygen with one of hydrogen, the other of a particle of oxygen with six of hydrogen, so great a variety of compounds may be generated, a question naturally arises, why may not the atoms of oxygen and hydrogen be capable of uniting in more than these two proportions, and why may not other kinds of matter, at present deemed simple, have also atoms of the same order, but of different numbers of the same two sorts of elementary atoms? Should this notion be adopted, it would follow that, as azote, a compound body, has hitherto resisted all attempts at decomposition, even in the most powerful voltaic circuits, chlorine, fluorine, boron, and other undecompounded bodies, may also be compounds of hydrogen and oxygen. There is nothing in the supposition but what is extremely probable, and what naturally suggests itself as a natural consequence of such modes of combination: their high specific gravity, and their general habits, certainly favour this idea. It is possible that all kinds of matter may be formed of these two sorts of ultimate substances. If chemistry should ever arrive at so happy a state of simplicity, how infinitely more grand must be the constitution of the universe than ever has been conceived in the most extreme periods of our enthusiasm. Following this view of the subject, electro-chemical habits will be more readily conceived. Water cannot arrange itself at either pole, because there exists in it an equal balance of attractive forces. Azote is strongly electro-negative, because it contains a preponderating force of negative atoms; it is consequently not affected in the circuit. Chlorine may be formed of a certain number of atoms of oxygen arranged round one of hydrogen; and hence, possessing strong electro-positive powers, will also remain unchanged in the highest voltaic arrangements. We need not wonder, then, that those atoms of the second class which possess an unequal balance of electro-chemical powers should remain unaltered in highly excited circuits, and that bodies composed of atoms of three series should so easily be deranged: because as the particles are increased in the number of their series, so will the outer ranges comparatively possess less influence; their order will therefore be more easily disturbed by other more powerfully attracting forces. The field is now open for all who feel interested in this enchanting pursuit; the extent of research is boundless beyond conception; and there may probably be gained by the beautiful system of atomic combination a more certain and accurate view into the secret operations of nature than has been obtained by all the valuable discoveries that have enriched the science of chemistry of late years.

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* Phil. Mag. vol. xlii. p. 266, *et seq.*

† Annals, vol. ii. p. 363, *et seq.*

These observations are intended, with much deference, merely as hints, or rather as the crude reflections naturally flowing from an early consideration of the subject. To others far more competent must be left the prosecution of this important task. As yet, however, it is too early a period for speculation; our knowledge is not sufficiently expanded to enter too far into the ordinances of the universe. We have only steadily to pursue the course so successfully led by the many great men of the present day, especially that so admirably marked out by your exertions, and by aiming at those objects only within our reach, we cannot fail of making gradual but rapid advancements to the objects of our exertions.

To conclude, I have only to regret that my professional avocations allow me but little opportunity of making much progress in my researches; what little leisure I can spare shall be most earnestly devoted to this favourite object; and as soon as I can collect materials sufficiently worthy of your observations, I will immediately transmit you the results of my humble efforts.

I am, Sir, with the most profound respect,
Your obedient servant,

111, *Strand, Feb.* 12, 1814.
JOHN MIERS.

ARTICLE VII.

Astronomical and Magnetical Observations at Hackney Wick. By Col. Beaufoy.

(To Dr. Thomson.)

MY DEAR SIR, Hackney Wick, April 17, 1814.

I HAVE the pleasure to send you the magnetical observations which will complete the twelve months' series, as well as the commencement of the second year's. I have only to remark, that every observation was made by myself, and that the most scrupulous attention has been paid to the subject. Fourteen observations were generally made with each needle, in the following manner: seven readings off from the instrument were set down, the needle was then drawn by applying a key or other piece of iron; and when the needle was settled seven more readings off were set down, and the mean of the fourteen observations was considered as the true variation of one of the needles. The experimented needle was then removed, and another placed in the box of the instrument, and fourteen observations were made in a similar manner; the mean of both needles was considered as the true variation. It may be proper to remark that needle number four weighs 48 grains, and needle number five 65¼; and that the former points out more readily any alteration or change in the magnetical fluid; and very possibly a still lighter needle might have greater advantages, as needles one, two, and three, which were heavier, did not answer so well.

I remain, my dear Sir, yours faithfully,

MARK BEAUFOY.

◆

Latitude, 51° 32′ 40·3″ North. Longitude West in Time $6''\frac{89}{100}$.

March 23, Emersion of Jupiter's	8h	23′	58″	Mean Time at H.W.
4th Satellite	8	24	04	Ditto at Greenwich.
April 5, Immersion of Jupiter's	7	23	55	Mean Time at H.W.
3d Satellite	7	24	01·8	Ditto at Greenwich.
April 7, Emersion of Jupiter's	8	36	15	Mean Time at H.W.
2d Satellite	8	36	21	Ditto at Greenwich.
April 12, Immersion of Jupiter's	11	23	48	Mean Time at H.W.
3d Satellite	11	23	54	Ditto at Greenwich.
April 14, Emersion of Jupiter's	11	14	13	Mean Time at H.W.
2d Satellite	11	14	19·8	Ditto at Greenwich.
April 14, Emersion of Jupiter's	11	57	03	Mean Time at H.W.
1st satellite..................	11	57	09·8	Ditto at Greenwich.

Magnetical Observations.

1814.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
March 18	8h	50′	24°	12′	58″	1h	55′	24°	19′	47″	6h	00′	24°	15′	20″
Ditto 19	8	55	24	14	05	1	50	24	21	39	6	15	24	15	52
Ditto 20	8	55	24	13	42	1	25	24	25	28	—	—	—	—	—
Ditto 21	8	55	24	12	37	—	—	—	—	—	—	—	—	—	—
Ditto 22	8	55	24	15	03	1	55	24	25	38	6	15	24	17	16
Ditto 23	8	55	24	13	01	1	45	24	24	35	6	15	24	11	02
Ditto 24	8	40	24	14	31	2	20	24	24	15	6	05	24	17	48
Ditto 25	8	55	24	15	44	1	52	24	22	06	6	05	24	15	34
Ditto 26	8	50	24	12	58	1	55	24	22	48	—	—	—	—	—
Ditto 27	8	50	24	12	45	1	50	24	23	26	6	08	24	17	34
Ditto 28	8	50	24	12	11	1	55	24	25	44	6	15	24	17	04
Ditto 29	8	50	24	12	28	1	50	24	22	47	6	15	24	16	28
Ditto 30	—	—	—	—	—	1	50	24	23	03	6	15	24	11	32
Ditto 31	8	38	24	12	20	—	—	—	—	—	6	15	24	15	27

Mean of Observations in March.	Morning	at	8h	52′.....	Variation	24°	14′	29″	West.
	Noon	at	1	52	Ditto	24	23	08	
	Evening	at	6	11	Ditto	24	15	33	
Ditto in Feb.	Morning	at	8	47	Ditto	24	14	50	West.
	Noon	at	1	52	Ditto	24	20	58	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at	8	52	Ditto	24	15	05	West.
	Noon	at	1	53	Ditto	24	19	03	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Dec.	Morning	at	8	53	Ditto	24	17	21	West.
	Noon	at	1	53	Ditto	24	19	49	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
[oittD in Nov.	Morning	at	8	42	Ditto	24	17	42	West.
	Noon	at	1	54	Ditto	24	20	24	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	Ditto	24	15	41	West.
	Noon	at	1	59	Ditto	24	22	53	
	Evening	at	—	—	Ditto	—	—	—	Not obs.

Ditto in Sept.	Morning	at	8^{h}	53′	..... Variation	24°	15′	46″	West.
	Noon	at	2	02	..... Ditto	24	22	32	
	Evening	at	6	03	..... Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	..... Ditto	24	15	58	West.
	Noon	at	2	02	..... Ditto	24	23	32	
	Evening	at	7	05	..... Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	..... Ditto	24	14	32	West.
	Noon	at	1	50	..... Ditto	24	23	04	
	Evening	at	7	08	..... Ditto	24	13	56	
Ditto in June.	Morning	at	8	30	..... Ditto	24	12	35	West.
	Noon	at	1	33	..... Ditto	24	22	17	
	Evening	at	7	04	..... Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	..... Ditto	24	12	02	West.
	Noon	at	1	37	..... Ditto	24	20	54	
	Evening	at	6	14	..... Ditto	24	13	47	
Ditto in April,	Morning	at	8	31	..... Ditto	24	09	18	West.
	Noon	at	0	59	..... Ditto	24	21	12	
	Evening	at	5	46	..... Ditto	24	15	25	

Magnetical Observations continued.

Month.		Morning Observ.					Noon Observ.					Evening Observ.				
		Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
Apri	1	8^{h}	38′	24°	10′	41″	1^{h}	50′	24°	23′	42″	6^{h}	15′	24°	16′	28″
Ditto	2	8	45	24	11	39	1	50	24	24	46	6	15	24	15	37
Ditto	3	8	45	24	13	04	2	15	24	24	00	6	25	24	14	48
Ditto	4	8	45	24	12	01	1	45	24	24	30	—	—	—	—	—
Ditto	5	8	45	24	12	28	1	50	24	24	46	6	25	24	09	35
Ditto	6	8	45	24	14	13	1	45	24	24	15	6	25	24	15	37
Ditto	7	8	48	24	12	47	—	—	—	—	—	6	25	24	16	21
Ditto	8	8	40	24	11	48	1	42	24	23	41	6	25	24	11	49
Ditto	9	8	55	24	12	55	1	55	24	25	14	—	—	—	—	—
Ditto	10	8	40	24	12	37	1	45	24	25	09	6	35	24	13	26
Ditto	11	8	45	24	12	00	1	35	24	24	16	6	35	24	14	03
Ditto	12	8	55	24	13	59	1	50	24	22	34	6	25	24	16	58
Ditto	13	8	45	24	12	48	1	50	24	23	20	6	30	24	18	13
Ditto	14	8	50	24	13	34	1	55	24	28	21	6	25	24	17	24
Ditto	15	8	35	24	11	23	1	45	24	22	04	6	25	24	15	05
Ditto	16	8	45	24	12	27	1	45	24	23	46	6	20	24	14	54
Ditto	17	8	50	24	12	25	1	30	24	24	49	6	30	24	15	28

April 2.—The highest needle, number four, vibrated at intervals 17 minutes, which is the greatest quantity observed since the commencement of the observations. This extraordinary fluctuation was in a few hours followed by a hard gale, with violent squalls and rain from the S. W.

The variation at noon on the 14th of April was unusually great; in the evening it blew strong from the S, W.; and the next day there was thunder from the same quarter.

Rain fallen { Between noon of the 1st Mar. / Between noon of the 1st Apr. } 0·875 inches.

ARTICLE VIII.

On the Daltonian Theory of Definite Proportions in Chemical Combinations. By Thomas Thomson, M.D. F.R.S.

(*Continued from* p. 134.)

In preceding numbers of the *Annals of Philosophy* I have given tables of the sulphates and nitrates as far as the experiments hitherto made upon these bodies warrant us to go.

It appears from the experiments of Berzelius that the *sulphites* correspond exactly in their composition with the *sulphates*. Hence the table given already for the sulphates will answer for the sulphites likewise. We have only to substitute the number denoting an integrant particle of sulphurous acid for the number which denotes an integrant particle of sulphuric acid, or, which comes to the same thing, we may subtract 1 from the number denoting the weight of an integrant particle of each salt.

The constituents of the nitrites have been too imperfectly examined to enable us to draw up a table of these salts. A few of them have been lately examined by Berzelius and Chevreuil; but the greater number are still unknown.

I shall now give a table of the *carbonates*, a genus of salts of considerable importance, and which have been examined with a good deal of care.

GENUS III.—*Carbonates.*

		Number of atoms.	Weight of an integrant particle.
226	Bicarbonate of potash	2 c + 1 p	11·502 [a]
227	Carbonate of potash	1 c + 1 p	8·751 [b]
228	Carbonate of soda	3 c + 1 s	16·135 [c]
229	Subcarbonate of soda	2 c + 1 s	13·384 [d]

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[a] According to this statement the salt ought to be a compound of 47·835 acid + 52·165 base. Now Berthollet's analysis gives us 47·64 acid + 52 36 base. (Jour. de Phys. lxiv. 174.) The salt is the common crystallized carbonate of the shops.

[b] Dr. Wollaston has shown that this salt contains just half the acid that exists in the bicarbonate.

[c] According to this statement it ought to consist of 39 acid + 37·247 base. Now Klaproth found it a compound of 39 acid + 38 base.

[d] According to this statement the salt ought to be composed of 16 acid + 22·921 base. Now Bergman obtained 16 acid + 20

		Number of atoms.	Weight of an integrant particle.
230	Bicarbonate of ammonia	2 *c* + 1 *a*	7·437 [e]
231	Carbonate of ammonia	1 *c* + 1 *a*	4·686
232	Subcarbonate of ammonia	1 *c* + 2 *a*	6·621 [f]
233	Carbonate of lime	1 *c* + 1 *l*	6·371 [g]
234	Carbonate of barytes	1 *c* + 1 *b*	12·482 [h]
235	Carbonate of strontian	1 *c* + 1 *st*	9·651 [i]
236	Bicarbonate of magnesia	2 *c* + 1 *m*	7·870 [k]
237	Carbonate of magnesia	1 *c* + 1 *m*	5·119 [l]
238	Carbonate of yttria	1 *c* + 1 *y*	11·151 [m]
239	Carbonate of zirconia	1 *c* + 1 *z*	1·407 [n]
240	Carbonate of glucina	1 *c* + 1 *g*?	6·351 ?
241	Carbonate of silver	1 *c* + 1 *s*	16·369 [o]

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base, Darcet 16·04 acid + 20·85 base, and Klaproth 16 acid + 22 base. These analyses very nearly coincide with my statement.

[e] This supposes the salt a compound of 100 acid + 35·169 base. Now Schrader found it a compound of 100 acid + 33·92 base, and Berthollet of 100 acid + 36·36 base.

[f] This approaches the analysis of Bergman, but does not coincide. The existence of the salt called carbonate of ammonia in the table has not been ascertained.

[g] This supposes the salt composed of 43·18 acid + 56·82 base. Now Dr. Marcet obtained 43·9 acid + 56·1 base.

[h] This supposes the salt a compound of 22·04 acid + 77·96 base. Now Kirwan obtained 22 acid + 78 base, and Berzelius 21·6 acid + 78·4 base.

[i] According to this statement the salt is a compound of 28·505 acid + 71·495 base. Now Klaproth's analysis gives us 30 acid + 69·5 base.

[k] This supposes the salt composed of 69·911 acid + 30·089 base. Now Kirwan and Fourcroy found it composed of 66·6 acid + 33·3 base.

[l] This supposes it a compound of 34 acid + 29·2 base. But no such compound is known. The carbonated magnesia of commerce approaches most nearly to a compound of 1 *c* + 2 *m*.

[m] This corresponds exactly with Klaproth's analysis, but no stress can be put upon it, because it was from that analysis that the number for *yttria* in my first table was derived.

[n] We have no analysis of this salt. Vauquelin found it a compound of 55·5 zirconia + 44·5 acid and water. The numbers in the table suppose it composed of 26·995 acid + 55·5 base. It would be correct if Vauquelin's carbonate contained 17·505 per cent. of water.

[o] This supposes carbonate of silver a compound of 16·806 acid

		Number of atoms.	Weight of an integrant particle.
242	Percarbonate of mercury	1 *c* + 2 *m*	...... 56·751 [p]
243	Percarbonate of copper..	1 *c* + 1 *c*	...... 12·751 [q]
244	Carbonate of iron	2 *c* + 1 *i*	...... 14·168 [r]
245	Carbonate of lead......	2 *c* + 1 *l*	...... 33·476 [s]
246	Carbonate of nickel	2 *c* + 1 *n*	...... 14·804 [t]
247	Carbonate of zinc	1 *c* + 1 *z*	...... 7·890 [u]
248	Carbonate of manganese	2 *c* + 1 *m*	...... 13·632 [x]
249	Carbonate of cerium....	2 *c* + 1 *ce*	...... 18·996 [y]
250	Percarbonate of cerium ..	3 *c* + 1 *ce*	...... 21·747 [z]

These 24 are all the carbonates with which we are at present acquainted. Most of the other metals do not seem capable of forming carbonates. Upon examination it will be found that all

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+ 83·194 base. Now Berzelius found it composed of 15·9 acid + 84·1 base. (Lärbok i Kemien, ii. 398.)

[p] Though this nearly agrees with the analysis of Bergman, yet little stress can be put upon it.

[q] This supposes the salt composed of 19·73 acid + 71·719 peroxide. Now Berzelius found it composed of 19·73 acid + 71·7 peroxide. (Lärbok i Kemien, ii. 333.)

[r] According to the analysis of Klaproth this salt is composed of 38·3 acid + 61·6 base. Now the statement in the table supposes it a compound of 38·3 acid + 60·325 base.

[s] This agrees exactly with the analysis of Berzelius, and very nearly with that of all other chemists. It supposes the salt a compound of 16·446 acid + 83·554 deutoxide of lead. Now Berzelius obtained 16·444 acid + 83·333 oxide. (See *Annals of Philosophy*, vol. iii. p. 60.)

[t] This statement comes nearest to the analysis of Proust, but does not agree with it completely. Proust found carbonate of nickel composed of 100 acid + 173·5 oxide. The statement in the table makes it a compound of 100 acid + 169·12 oxide. The difference scarcely exceeds 2 per cent.

[u] According to Mr. Smithson this carbonate is composed of 1 acid + 2 oxide. Now the statement in the table supposed it a compound of 1 acid + 1·868 oxide.

[x] This supposes the salt a compound of 34·16 acid + 50·476 oxide. Now John's analysis gives us 34·16 acid + 55·84 oxide. This does not correspond well. Hence the number in the table is doubtful.

[y] This supposes the salt composed of 22·7 acid + 55·545 deutoxide. Now Hisinger's analysis gives us 22·7 acid + 57·9 oxide.

[z] This supposes the salt to be composed of 36·17 acid + 72·28 peroxide. Now Hisinger's analysis gives us 36·17 acid + 63·83 peroxide.

the carbonates in the table correspond with the canon of Berzelius, that the oxygen in the base is either equal to that in the acid or a submultiple of it by a whole number. It was more likely that this rule would hold with the carbonates than with the sulphates or nitrates, because carbonic acid contains only two atoms of oxygen, and the multiples of this number are much more likely to occur constantly than of three, the number of atoms of oxygen which exist in sulphuric, or five, that of the atoms in nitric acids.

From the preceding table it appears probable that most of the carbonates have been accurately analyzed, for almost the whole of them agree very nearly with the numbers contained in the table.

It follows likewise from this table that 100 grains of carbonic acid combine with a quantity of base containing 36·22 grains of oxygen, or at most 36·35 of oxygen. If we were to calculate the composition of the carbonates on that supposition, we should obtain results almost coinciding with those given in the table. This I consider as a farther corroboration of the accuracy of the constituents of these salts as here established.

(To be continued.)

Article IX.

Analyses of Books.

Philosophical Transactions of the Royal Society of London for the Year 1813, *Part II.*

This Part contains the following papers.

1. An Account of some Organic Remains found near Brentford, Middlesex. By the late Mr. William Kirby Trimmer. These organic remains were dug out of two fields: the first about half a mile north from the Thames at Kew Bridge, the surface of which is about 25 feet higher than the Thames at low water. The beds in this field beginning at the surface are the following:

		Thickness.
1.	Sandy loam	6 to 7 feet
2.	Sandy gravel	A few inches
3.	Loam, slightly calcareous	1 to 5 feet
4.	Peat in small detached patches	A few inches
5.	Gravel containing water Thickest under the peat.	2 to 10 feet
6.	The London clay	200 feet.

The first bed contains no animal remains; the second contains snail shells, river shells, and a few bones of land animals, so mutilated that the class to which they belong cannot be ascertained. The third bed contains the horns and bones of the ox, the horns, bones, and teeth of the deer, besides snail shells and shells of river fish. The fifth bed contains the teeth and bones of both the African and Asiatic elephant, the teeth of the hippopotamus, and bones, horns, and teeth of the ox. In the clay the animal remains are all marine, except some specimens of fruit and petrified wood. The other fossils are nautili, oysters, pinnæ marinæ, crabs, teeth and bones of fish, and a great variety of small marine shells.

The second field lies about a mile west from the first, and a quarter of a mile from the river Brent. It is 40 feet above the Thames at low water. Its beds are

		Thickness.
1.	Sandy loam	8 or 9 feet
2.	Sand becoming coarser towards the lower part	3 to 8 feet
3.	Sandy loam, highly calcareous	1 inch to 9 feet
4.	Gravel containing water	unknown
5.	London clay	unknown

The first bed contains no animal remains. In the second, but always within two feet of the third bed, have been found the bones and teeth of the hippopotamus, the teeth and bones of the elephant; the horns, bones, and teeth of several species of deer; the horns, bones, and teeth of the ox, and the shells of river fish. These bones must have been deposited in the state of detached bone. The gravel shows no marks of having been rounded by attrition. The third bed contains the horns, bones, and teeth of the deer, the bones and teeth of the ox, together with snail shells and the shells of river fish.

2. On a new Construction of a Condenser and Air Pump. By the Rev. Gilbert Austin. This is a very ingenious instrument; but it would be impossible without the assistance of plates to make it intelligible to the reader. The most important improvement introduced by Mr. Austin is fitting two pieces of glass to each other by plain surfaces, instead of making the one piece enter into the other. It is a method which I have frequently employed, and I always found it much more convenient than the common way of making glass vessels air tight.

3. On the Formation of Fat in the Intestines of living Animals. By Sir Everard Home, Bart. This hypothesis, that after the chyle has been separated from the food in the smaller intestines, it undergoes a further change in the lower intestines, being partly converted into fat, which is carried off by unknown channels, is founded on the following data: 1. Unless fat be formed in the lower intestines, no other source of it can be pointed out. This the author states as one of the strongest arguments in favour of his hypothesis. 2. Those birds which are but scantily supplied with food, have a prodigious length of colon, compared with those that have a copious supply. 3. The situation of the food in the colon is similar to that of muscular flesh in a stream of water, which is well known in such a situation to be converted into adipocire.

Hence it is inferred, that the food in the colon will undergo a similar change. I must own that I cannot very clearly perceive the analogy between the situation of the food in the colon, and of animal muscle in a stream of water, or buried in the ground and occasionally drenched with water. 4. Ambergris, which contains 60 per cent. of fat, is found in the lower intestines of the spermaceti whale, and solid masses of fat are sometimes formed in the human intestines. 5. Food confined for a week in the cœcum of a duck, was partially converted into fat by the action of dilute nitric acid; but no such change was produced by the same process on the contents of the rectum. 6. Bile when digested upon animal muscle gives it the smell of excrement. 7. Bile has the property of converting muscle into fat, when digested upon it at the temperature of 100°; but not at a lower temperature. 8. Human fæces long retained exhibit traces of fat when digested in hot water. 9. The want of bile seems entirely to prevent growth; for a child, destitute of a gall bladder, never grew, and died emaciated, though it took food and passed perfect fæces. Such are the premises from which our author draws his conclusions. They are certainly curious, and do honour to the industry and sagacity of Sir Everard Home; but the reasoning is so loose, that it could not be permitted in any other science. It shows very clearly the low state of physiology, and the small confidence that can be put in any of its departments. Were it not so, a man of Sir Everard Home's celebrity and sagacity would not have hazarded the founding of a theory upon such imperfect data.

4. On the colouring Matter of the black Bronchial Glands, and of the black Spots of the Lungs. By George Pearson, M.D. F.R.S. The human lungs at first are of a red colour, but as the person advances in age they acquire a mottled, and at last nearly a black colour; and the bronchial glands contain a black matter, which cannot be completely separated by maceration. This black matter was examined by Dr. Pearson. It is insoluble in nitric and all acids, except the sulphuric, in which it partially dissolves. It is insoluble in alkaline leys. It deflagrates with nitre and hyperoxymuriate of potash, and furnishes carbonic acid. From these and other similar experiments, Dr. Pearson considers it as charcoal, taken in with the air breathed; and derived from the sooty matter mixed with the air from the combustion of coal, &c.

5. Experiments on the Alcohol of Sulphur or Sulphuret of Carbon. By J. Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm; and Alexander Marcet, M.D. F.R.S. one of the Physicians of Guy's Hospital. A full account has been already given of this important and interesting paper in the *Annals of Philosophy*, vol. iii. p. 185.

6. On the means of procuring a steady Light in Coal Mines without Danger of Explosion. By William Reid Clanny, M.D. of Sunderland. The disasters occasioned by the explosion of carbureted hydrogen gas in coal mines have lately become more fre-

quent and more destructive than formerly, especially about Newcastle and its neighbourhood; or rather I believe they are not now so carefully concealed as they used to be. I consider the mode of ventilating the coal mines, at present practised, as exceedingly defective, and have not a doubt that they might be so contrived as to be kept perfectly free from all such accidents, without any additional expense to the proprietors. But I have little hopes of seeing such improvements attempted; because they are considered as injurious to the interest of certain persons at present employed in coal mines, who will have address enough to convince the proprietors of such mines that all attempts at new methods are absurd and nugatory. While the present very absurd mode is continued, Dr. Clanny's lamp, which is equally simple and ingenious, might be employed to diminish or destroy the danger of explosions. It is merely an air tight lantern, containing a candle, which is kept burning by means of a current of air blown through it by a bellows. If the air contains such a quantity of inflammable gas as to explode only the portion within, the lamp would burn, and thus the workmen would escape danger. Such lamps it is true would be much more expensive than the present mode; but they would save the lives of hundreds of workmen who at present fall victims to these destructive explosions.

7. On the Light of the Cassegrainian Telescope compared with that of the Gregorian. By Captain Henry Kater, Brigade Major. Major Kater was enabled to compare these two telescopes with each other, in consequence of the excellency at which a self-taught artist in Ipswich, named Crichmore, had arrived in making these instruments. Several of each, in every respect of the same goodness, were within the examination of Major Kater. The result was, that with an equal aperture, the light of the Cassegrainian telescope was to that of the Gregorian as 6 to 3·3. He conceives the difference to arise from this circumstance. In the Gregorian telescope the focus of the rays from the great mirror is at the small mirror, while in the Cassegrainian telescope the rays from the great mirror arrive at the small one before they reach their focus; hence in the first they cross each other, but not in the second. Now when thus crossing, they may interfere with each other, or they may repel each other, and thus occasion the dissipation of a quantity of the light.

8. Additional Observations on the Effects of Magnesia in preventing an increased Formation of Uric Acid; with Remarks on the Influence of Acids on the Composition of Urine. By William Thomas Brande, Esq. F.R.S. Prof. Chem. R.I. The use of magnesia when the urine deposites red sand, or has a tendency to form calculi composed of uric acid, was first suggested by Mr. Hatchett; and it appears from two cases related in this paper, that it is attended with the happiest effects. It does not prove injurious to the stomach, nor does it excite irritation in the bladder, as is the

case with the alkalies. But it appears, that when persevered in improperly, after the tendency to deposite uric acid is removed, it occasions a deposit of white sand in the urine, consisting of phosphate of magnesia-and-ammonia, and phosphate of lime; and thus gives origin to the formation of a new kind of calculus. In all such cases, Mr. Brande shows that the use of magnesia must be intermitted, and acids substituted in its stead. Muriatic, nitric, and sulphuric acids remove the tendency to deposite white sand; but they are apt to occasion disorder in the stomach, and to produce irritation in the bladder. Vegetable acids correct the state of the urine without being so apt to produce disagreeable symptoms. Vinegar and citric acid were used successfully by Mr. Brande, and carbonic acid was found to answer best of all; as it had no tendency to injure the stomach or irritate the bladder, while it removed the tendency in the urine to deposite a white sand.

9. Additions to the Account of the Anatomy of the Squalus Maximus, contained in a former Paper; with Observations on the Structure of the Bronchial Artery. By Sir Everard Home, Bart. F.R.S. This paper consists of additions to and corrections of the former paper on the same subject. In the former figure, a small fin situated between the anus and tail was omitted, which induced naturalists to suppose that the fish described was a peculiar species. The pylorus portion of the stomach is very long and narrow. The bronchial artery is muscular. This the author shows is intended to regulate the flow of blood to the gills, when the fish is at different depths. Fishes have no cerebrum, but only a cerebellum and medulla oblongata. The medulla oblongata of the squalus maximus is very large.

10. Some further Observations on a new detonating Compound. By Sir Humphry Davy, LL.D. F.R.S. V. P. R. I. This substance, which Sir H. Davy proposes to call *azotane*, was discovered by M. Dulong in France, but he did not investigate its properties. It may be obtained by exposing dilute solutions of nitrate of ammonia or sal ammoniac to chlorine gas. It is a brown coloured oily looking substance, very volatile, and it does not congeal when exposed to the cold, produced by a mixture of snow and muriate of lime. Its specific gravity is 1·653. It gradually disappears in water, azote being evolved and nitro-muriatic acid formed. When put into concentrated liquid muriatic acid it disappears, and a quantity of chlorine is evolved, considerably exceeding it in weight, at the same time sal ammoniac is formed. In concentrated nitric acid it gives out azote. In diluted nitric acid it gives a mixture of azote and oxygen. It detonates in strong solutions of ammonia; in weak solutions it gives out azote. It dissolves in sulphurane, phosphorane, alcohol of sulphur, and fluoric acid, without any violent action. When exposed to pure mercury azote is evolved, and a white powder formed consisting of a mixture of calomel and corrosive sublimate. By a very ingenious analysis, but on too small a

scale to be depended on as exact, Sir H. Davy ascertained, that it is a compound of one volume of azote and four volumes of chlorine, or by weight of

Chlorine	90·940	or	4·525 × 4
Azote	9·060	or	1·803
	100·000		

11. Experiments on the Production of Cold by the Evaporation of Sulphuret of Carbon. By Alexander Marcet, M.D. F.R.S. one of the Physicians to Guy's Hospital. Dr. Marcet has ascertained, that a greater degree of cold is produced by the evaporation of sulphuret of carbon than by that of any other liquid. If the bulb of a small spirit of wine thermometer be surrounded with lint wetted with alcohol of sulphur, it sinks in the open air even in summer nearly to zero. If it be introduced into the receiver of an air pump, and a vacuum be made, the thermometer in a few minutes sinks to — 70° or — 80°; and if a tube containing mercury be substituted in its place, that metal freezes very speedily.

12. On a Saline Substance from Mount Vesuvius. By James Smithson, Esq. F.R.S. This saline substance was thrown out of Vesuvius about the year 1792, and was examined by Mr. Smithson in 1794. A more recent examination, described in the paper, but not susceptible of abridgment, enabled him to ascertain its composition to be as follows:

Sulphate of potash	7·14
Sulphate of soda	1·86
Common salt	0·46
Sal ammoniac Muriate of copper Muriate of iron	0·54
	10·00

There was mixed with it also some submuriate of copper and submuriate of iron.

13. Some Experiments and Observations on the Substances produced in different Chemical Processes on Fluor Spar. By Sir Humphry Davy, LL.D. F.R.S. V.P.R.I. This paper is very different from the one read to the Royal Society on the 8th July, 1813; being much fuller, and containing a greater number of experimental details. Three substances have been for a considerable time known to chemists; namely, fluoric acid, silicated fluoric acid, and fluoboric acid. The two first of these were discovered by Scheele; the last by Gay-Lussac and Thenard. Sir H. Davy's attention was drawn to this subject by a letter from M. Ampere, who drew a comparison between fluoric acid and muriatic acid, and endeavoured to show that the former like the latter was a compound of hydrogen, and an unknown supporter of combustion, for

which he suggested the name of *fluorine*. This hypothesis was adopted by Sir H. Davy; and the object of the present paper is to state the facts which can be brought forward in its support.

Liquid fluoric acid was first obtained pure by Gay-Lussac and Thenard. It is procured by heating concentrated sulphuric acid and fluor spar, in retorts of silver or lead, and receiving the products in receivers of the same metals artificially cooled. It is a very active substance, and requires to be examined with great caution. Davy considers it as pure acid without any water. Its specific gravity is 1·0609. When mixed with water a great deal of heat is evolved, and if the water be cautiously added, the specific gravity increases to 1·25.

Fluoboric acid is a gas which may be obtained by heating in a glass retort a mixture of fluor spar, boracic acid, and sulphuric acid. Its specific gravity is 2·370. It forms a solid volatile salt with its own bulk of ammoniacal gas.

According to the hypothesis of Ampere and Davy, fluoric acid is a compound of fluorine and hydrogen; silicated fluoric acid, of fluorine and silicon; and fluoboric acid, of fluorine and boron. Fluor spar is a compound of fluorine and calcium; and so of the other compounds. The evidence brought forward by Sir H. Davy in favour of this hypothesis is as follows: 1. Liquid fluoric acid lets go no water when combined with ammonia, as is the case with sulphuric, nitric, phosphoric, and all the acids containing oxygen. 2. When fluate of ammonia and potassium are heated, fluate of potash is formed, and ammoniacal gas and hydrogen emitted in the proportion of two measures of the first to one measure of the second. This favours the notion that fluate of potash is a compound of fluorine and potassium; the hydrogen being produced by the decomposition of the fluoric acid. Potassium when heated with sal ammoniac gives a similar result. 3. When galvanic electricity is made to act upon liquid fluoric acid, the platinum wire at the positive pole is corroded, and deposites a chocolate powder, while hydrogen only is given out at the negative pole. Now if it had contained any other inflammable basis besides hydrogen, analogy would lead us to expect that it would have been given out along with the hydrogen; but this experiment was too imperfectly made to be considered as conclusive.

These analogies led Sir H. Davy to endeavour to separate fluorine from the fluates, by heating them in chlorine or oxygen; but all his attempts failed. In glass vessels the glass was violently acted upon, and silicated fluoric gas and oxygen evolved. In platinum vessels that metal was acted on, and a red powder formed. Nor was the attempt to decompose liquid fluoric acid by passing it along chlorine through red hot tubes more successful.

These experiments are far from demonstrating the truth of the hypothesis of Ampere and Davy; though analogy is certainly in its favour.

14. Catalogue of North Polar Distances of eighty-four prin-

cipal fixed Stars, deduced from Observations made with the Mural Circle at the Royal Observatory. By John Pond, Esq. Astronomer Royal, F.R.S. The observations from which this catalogue was drawn up appear to have been made with great care; and are so numerous, that the distances are certainly made out much more exactly than in any former tables. But from the very nature of such a catalogue, which occupies 20 pages, we are precluded from giving it here.

15. Observations on the Summer Solstice of 1813, with the Mural Circle, at the Royal Observatory. By John Pond, Esq. Astronomer Royal, F.R.S. The mean obliquity of the ecliptic comes out from these observations 23° 27′ 49·5″. The mean obliquity at the summer solstice, 1812, was 23° 27′ 50·5″, and at the winter solstice, January 1st, 1813, it was 23° 27′ 50·0″.

ARTICLE X.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, the 24th March, a paper by Thomas Young, M.D. Foreign Secretary to the Royal Society, on the new structure of ships proposed by Mr. Seppings, was read. Dr. Y. began by observing, that the advantage of the oblique position of the beams and riders had been long known to men of science, and that various unsuccessful attempts had been made to introduce that position into ship building. He then calculates the strain upon ships of war from the length and weight, and the action of the waves. He shows that the oblique position of the beams and riders does not add to the total strength; but that it is an improvement, on account of the additional stiffness and inflexibility which it affords. He examines the different alterations made by Mr. Seppings, and points out those which he considers as improvement and those respecting the advantage of which he is doubtful. Though, from the clearness and precision with which this paper was written, it would be easy to give a pretty full analysis of it, I am induced to abstain from proceeding any farther, from an apprehension that the analysis would scarcely be understood without a more detailed account of Mr. Sepping's paper than I was able to give from hearing it read.

On Thursday, the 31st March, a paper by Mr. Groombridge was read, containing additional observations on atmospherical refraction. In his former paper he had confined his observations to stars not more than 70° from the zenith. He has since gone a good deal farther. The result is, that Bradley's formula, with certain alterations in the value of some of the quantities, will apply to all stars not more than 85° from the zenith, but beyond that distance a new formula is necessary.

At the same meeting a paper by Mr. Hay was read, on certain properties of tangents, of circles, and of trapeziums inscribed in circles.

On Thursday, the 21st April, a paper by Dr. Brewster, on the optical properties of mother-of-pearl, was read. An outline of the curious facts contained in this paper was given in the last number of the *Annals of Philosophy*.

When we look at the image of a candle reflected from the surface of a piece of regular mother-of-pearl, ground but not polished, we perceive at the distance of four or five degrees from the common image a highly coloured image, the distance of which from the common image increases with the angle of incidence. By polishing the mother-of-pearl a new image exactly like the first, and obedient to the same laws, is developed on the other side of the common image. These optical properties may be communicated by pressure to wax, cement, gum arabic, balsam of Tolu, realgar, tin foil, the amalgam of bismuth, and even to lead. Hence it follows that the optical properties of mother-of-pearl are owing to a certain configuration of the surface, which cannot be removed by the finest polishing. By examining the surface of mother-of-pearl by means of microscopes, he found that it was composed of grooves similar to the skin at the point of an infant's finger. These grooves are very fine. The distance between them varies. Sometimes they may be seen with the naked eye; sometimes there are about 3000 in the inch. It is to this grooved structure that mother-of-pearl is indebted for its optical properties.

LINNÆAN SOCIETY.

On the 5th April there was read a tabular view of four classes of animals, considered by Linnæus as constituting but one class, by William Elford Leach, M. D. The principal object of this paper is to call the attention of entomologists to examine into the propriety of constituting a new class of animals to comprehend the classes *syngnatha* and *chilognatha* of Fabricius, which Latreille and Lamarck have arranged with the *arachnides*.

On the 19th April a paper, also by Dr. Leach, was read, on the distribution of the crustacea into orders, in which the *entomocrasta* and *myriapoda* of Müller are considered as forming sub-classes. He proposes to divide the *malacostraca* into three orders, two of which with pedunculated eyes are distinguished from each other by the form and proportion of the tail: the third order, with sessile eyes, he admits to be artificial.

Some observations by the President on Brodel's remarks upon the subject of Mr. Dickson's work on mosses, were also read.

GEOLOGICAL SOCIETY.

On the 21st January a letter from Mr. Greenough was read, relating the result of an examination made by himself, in company with Mr. Irton and Mr. Buckland, concerning the sand tubes that have been found near Drigg. Mr. Irton had before made an unsuccessful sinking to the depth of several feet in the loose sand, for the purpose of ascertaining the termination of these tubes. When Mr. Greenough was on the spot last autumn the favourable state of the drift sand allowing him to commence a fresh excavation at a lower level than that at which Mr. Irton had been obliged to leave off, he diligently availed himself of it, and the following is the result. After tracing the tube in a perpendicular direction through the loose sand to the depth of six feet, they arrived at the pebbly beach on which the hillock stands; here the tube was found adherent to a pebble of hornstone porphyry, the surface of which appeared to show evident signs of partial fusion. From the surface of this pebble the tube glanced off, at first obliquely, but soon resumed its original direction, and at the depth of about a foot below the pebble was lost. From these circumstances the formation of the tube is attributed to lightning.

A paper by Mr. Warburton relative to some beds of shell marl in Scotland, compiled from documents furnished by Mr. Lambert, of Cambridge, was read. These beds of shell marl occur for the most part in the shire of Angus, and occupy shallow basons in a red sandstone rock, being at present covered either by peat or by water, and not unfrequently by both. The thickness of the marl towards the centre of the bason sometimes exceeds ten feet; but as it approaches the circumference, it gradually thins out, so as not to amount to more than a few inches in depth. Sometimes these beds are single, in other places a succession of them is met with. The peat mosses of Glamis and Forfar afford the following series, beginning with the uppermost. Moss, containing trees, from four to six feet; shell marl, from six to seven feet; blue clay; shell marl, nine inches; gravel or quicksand; shell marl. The marl appears to consist wholly of shells of the same species as at present inhabit the water by which it is covered; namely, the helix putris, and cardium amnicum chiefly entire, and the mytillus cygneus for the most part in fragments. Thus therefore the formation of calcareous beds with alternations of sand and clay appears to be one of those natural processes which even yet has not ceased to operate.

An account of the Swedish corundum, by M. Swedenstierna, was read. This mineral has hitherto been found only in the iron mines of Gellivara, in Lapland, where it occurs very sparingly, imbedded in a massive variety of iron glance, accompanied by red felspar, red and greyish-white apatite, and silvery mica. As yet it has been met with only in crystals of a light smoke-grey colour, in the form of an oblique octohedron, sometimes regular, but often so much compressed as to exhibit to the naked eye only two large faces, the lateral ones being almost imperceptible. The size of the crystals varies, from that of a pin's head to a length of three lines or more.

On the 18th February a paper by Dr. M'Culloch, on vegetable remains preserved in chalcedony, was read.

Arborizations in chalcedony are of by no means unfrequent occurrence. Sometimes they are so distinct as at once to command attention; but often, from the minuteness of their ramifications, or from their being excessively crowded together, they are considered as mere stains, and thus elude superficial observation.

The whole may be divided into three classes. The first will comprehend those which from their external form, and the perfection of their internal organization, will almost universally be acknowledged by competent judges to be undoubted vegetables. Their colour is generally green or blackish-brown, and they belong to the families of conferva, lichen, and the musci. The second class includes those which are invested with a crust of carbonate or oxide of iron, and in which the structure can only be observed in the transverse sections, and of which therefore the real origin is not so certain as in those that belong to the first class. Their colours are various; red, dingy purple, and ochre yellow, are the principal.

The third class includes those in which the vegetable form and structure are more or less perfectly imitated by grains and crystals of chlorite, and by metallic oxides, but which, however accurate the resemblance, are truly pseudomorphous. Their colours very generally agree with those of the first class.

The assistance of chemical agency was also had recourse to, for the purpose of still farther confirming the reality of the distinctions abovementioned. It was found that agatized wood was blackened by immersion in sulphuric acid, but that no such effect was produced on chalcedony or on chlorite; a certain number of specimens of the first and third classes were then immersed in boiling sulphuric acid, and the result was, that the fibres of real vegetable origin became black, while those which consisted of chlorite did not become black, but effervesced very sensibly.

How did these vegetables become enveloped in a mass of chalcedony? It is well known that the confervæ retain their green colour only while they are alive; and as in many of these specimens not only the colour but the free unconstrained attitude of the plant is perfectly preserved, the theory most consonant with actual appearances seems to be, that the plants were involved in an aqueous solution of silex so dense as to be capable of speedily gelatinizing, and in which they were preserved from further change while the conversion of this jelly into hard stone was taking place.

CORNWALL GEOLOGICAL SOCIETY.

At a meeting of this Society, on the 9th of April, a paper was communicated by Ashurst Magendie, Esq. upon the occurrence of granitic veins in *argillaceous schistus* at Thouschole. At this point of the coast he observed the schistus to terminate, and the granite to commence. The schistus is of a greyish colour, rather hard, but breaks in large fragments in the direction of the strata: the granite is of a fine grain, and the felspar is of a light flesh colour, and contains but a small proportion of mica. At the junction numerous veins of granite may be traced from the rock of granite, into the schist. Some of these veins may be observed upwards of 50 yards, till they are lost in the sea; and in point of size vary from a foot and a half to less than an inch. It may deserve notice that as the felspar is of a flesh colour, it is impossible for any observer to consider them as quartz veins: one of these large veins is dislocated and heaved several feet by a cross course of quartz, and fragments of schistus having the appearance of veins are found in the granite veins. At one place the author observed a very curious and satisfactory phenomenon. He found that one of these veins of granite, after proceeding vertically some distance, suddenly formed an angle, and continued in a direction nearly horizontal for several feet with schistus both above and below it, which appearance most completely destroys one of the theories suggested for the explanation of similar veins at St. Michael's Mount, viz. that a ridge of granite had been left, and clay slate deposited afterwards on its sides.

At the same meeting a paper from Mr. Joseph Carne, Esq. was read, giving a more ample account of the Relistian Mine than that published in the Philosophical Transactions, accompanied by some interesting sections, and a plan of its lodes.

At this meeting also some account of the sand found at Piran was described by Dr. Paris, the Secretary to the Society. It appears that Nature is actually at this time, by some mysterious process, converting this sand into stone, and specimens of it are found in different states of induration. A further account of this very interesting subject will be given in a future number of the Journal.

We are happy to state that this useful Society is very considerably increased in numbers since our last report of it, having already inrolled the names of more than an hundred active and intelligent members.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute of France during the Year 1813.

Memoir of M. Burckhardt on the Quantity of Matter in the Planets.

(*Continued from p.* 309.)

On the small Equations which exist in the Theory of Jupiter.

The analytical calculus, notwithstanding the precision to which it has been brought by mathematicians, has still its imperfections as well as the best instruments. For want of direct methods, the equations can only be integrated by means of series, all the terms of which considered as sensible are taken, while the rest are neglected. In fact each term omitted is probably so small that if it were alone it might be very properly neglected; but the total number of these is so great that notwithstanding all the compensations that may be hoped for, it is not impossible but the total error may be perceptible. If this error continue for a considerable time, and if the variations seen in it be so slow as not to be ascribable to any of the equations employed in the tables, recourse is had to the combination of angles, whose periods are longer. What is troublesome in these attempts, instead of one of those combinations called *arguments*, several occur, and it is difficult to determine upon which one the choice should fall. In that case every thing is uncertain, the argument as well as the coefficient of the inequality sought. The observed error may be a combination of various inequalities equally unknown, which will not be brought into view for several centuries. One is therefore reduced to the necessity of trying them one after another, and thus engaging in endless calculation without any reasonable expectation of success. The combination adopted may agree with preceding observations, but there is little chance of their agreeing with future observations. Calculation has been lengthened, and the tables increased, without any demonstrated utility; but the more thorny this path is, the more praise is due to those astronomers who have the courage to enter upon it. Such has been the conduct of those astronomers who have attempted to ameliorate the lunar tables. But when the formula of the movement of a planet is given, when all the quantities composing it are known, or may be deduced from those that are known, then nothing more is wanted than patience to follow out all the developements, and to determine all the equations which ought to enter into the tables. This at least is the advantage which the formulas of the planetary disturbances present. Those for which we are indebted to the author of the *Mecanique Celeste* are given in his work, with an extent which appears more than sufficient for the inferior planets. Nothing is wanted but a more perfect knowledge of their quantity of matter; but the contrary is the case with the three superior planets. Their quantities of matter ought to be sufficiently known, since all the three have satellites; but their motions are slower, and their relations such that it becomes necessary to push the approximations to quantities of higher orders. To this, as well as to a want of observations, are owing the slight errors which have been found in the first tables of Jupiter and Saturn, printed about 22 years ago. Since that time M. Laplace has again reviewed his theory. By making slight corrections on the quantity of matter, by employing the observations made since that time, and by abandoning the observations of Flamsteed, and all those that preceded 1750, because they rather injure than ameliorate the tables, M. Bouvard had been able to make the calculations nearly agree with the modern observations. He has carried his hopes still farther, and is at present at work still more to diminish the errors. Under these circumstances M. Burckhardt, who had already given the analytical developements of the formulas of Laplace, has considered it as useful to consider what these formulas would give for the inequalities of Jupiter, as far as those of the sixth order inclusive. In the memoir which he has lately read to the Class we see equations of five different orders, from the second to the sixth, forming altogether 35 terms, the greatest of which does not amount to five seconds, and the sum total is 40·25″.

It is impossible, as we have already remarked, that these equations should all have the same sign, and be a maximum; but without seeking for the probable mean, let us suppose it to amount to 12″ or 15″. This is what will be gained by introducing these new equations into the tables of Jupiter. It is doubtful, as M. Burckhardt observes, whether these inequalities be more considerable for Saturn.

In making these useful additions to the tables of the superior planets, if we have not yet been able to reduce the errors to what may be ascribed to the observations, M. Burckhardt thinks that we may be certain of the influence of the small planets on the other celestial bodies; for he has taken the greatest care to render his labour complete.

Second Memoir on the Distribution of Electricity on the Surface of Conductors, by M. Poisson.

The author in his first memoir had given the equations for two spheres placed at any distance whatever from each other. He had shown how they might be reduced to ordinary equations with variable differences, and then to a single independent variable quantity. He had resolved them completely by two particular hypotheses. In the one he supposed the two spheres in contact, and in the other the distance between the two surfaces was very great when compared with the radii.

M. Poisson gives here the general integrals of his two equations, first in the form of a series, and then under a finite form by means of definite integrals. He proves that in all cases these formulas contain only quantities always determined by the state of the question. They express the thickness of the coat of electricity, or the intensity of the electricity in any point of one or other surface. Provided the spheres be not too near, the series will always converge so fast that as accurate values as we choose may be easily deduced. To show the use of them, he supposes two spheres, the first of which has a radius triple that of the second. He calculates the thickness of the coat in nine different points at equal distances upon the great circle whose plane passes through the line of the centres from the point where that line cuts the surface to the point diametrically opposite. In the table which he has formed we see the law according to which the electricity increases or decreases on each of these spheres. We see whether the electricity is positive or negative, and we can easily determine the point where the change of sign takes place.

We may make all the suppositions we please respecting the nature or the quantity of electricity with which each of the spheres is charged. If we make one of these quantities equal to zero, we shall have the case where one of the spheres is electrified by the sole influence of the other, and we well know the re-action of this sphere upon the former. If it is the smallest sphere that is electrified by the influence of the greatest, the electricity will diminish upon the greatest to 67° 30′ from the point nearest the little sphere, and it will then increase to the point diametrically opposite. The author points out the method of producing at pleasure a minimum of this kind. He draws from his formulas several other consequences equally curious, and which would deserve a new set of experiments to demonstrate their truth to those that are unable to follow his analysis. In the mean time what ought to give the fullest confidence in the accuracy of this theory is its astonishing agreement with all the experiments which M. Poisson could find in the works of philosophers.

We have said that the series cease to converge when the two spheres are very near each other; but by means of his definite integrals the author transforms them into other series, which, in order to converge, require that the distance between the two spheres be small. In this manner he determines what happens in the progressive approach of the two spheres before they touch each other, and what may be observed when after having placed them in contact they are again separated. The phenomena indicated by the calculus are precisely those observed by Coulomb, and this agreement appears to furnish a confirmation of the theory of two fluids at present somewhat hypothetical.

The case of two spheres leads to equations with variable differences, and with two independent variables. M. Poisson remarks that this is the first time that an equation of this kind has presented itself in the solution of a physical problem. This is a new proof that analytical researches, which one would be tempted sometimes to consider as simple objects of curiosity, always at last find a useful application.

(*To be continued.*)

Article XI.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Mr. Joseph Hopkins, surgeon to his Royal Highness the Duke of Kent, proposes to deliver a course of lectures on the theory and practice of midwifery, at the Westminster Lying-in Institution. The lectures are illustrated with cases, and each pupil is to deliver one patient every six days, beginning after he has attended the first ten lectures; and, for the accommodation of two classes, one course will commence with every month.

II. *Queries respecting the flowing of Water in Mines.*

The following letter from Mr. Moyle came to hand too late for insertion in the last Number of the *Annals of Philosophy*. As far as I understand the subject, the explanation proposed by Mr. Moyle seems satisfactory; but many of my readers are much more competent judges of such subjects than I am:

(To Dr. Thomson.)

SIR,

Your Philosophical Journal having become so extensively circulated, I conceive there cannot be a more fit medium to communicate to, or derive information from, your numerous readers; or probably *you* will do me the favour to set my conjectures at rest respecting a circumstance which occurred in Chacewater mine a few weeks since.

Chacewater is both a copper and tin mine, about 120 fathoms deep, in which is now working the most complete and largest steam engine ever known to have been built; at which depth there is a level at right angles, with the large perpendicular shaft (commonly called the engine shaft) several fathoms in extent, through which a great body of water flows to the engine shaft, to be drawn out. At 16 fathoms from the bottom runs another level from the perpendicular shaft, parallel with the bottom level; and in the course of the *load*, or vein, at the extremity of which there is sunk what the miners call a *wins*, which is another shaft perpendicular or inclining as the veins should *underlie*. When this *wins* was sunk about a fathom, the circumstance occurred to which I wish to draw your attention. When the engine ceased working for a few minutes, the bottom level became full of water, the springs instantly made their appearance (15 fathoms high) on the top of the *wins* in the upper level, and where it was perfectly dry before, and now emptied itself through this level into the engine shaft, where the water rose slow and progressively from the bottom.

Now what struck me so forcibly was, the sudden appearance of the water at such a height, while the reservoir below remained in a great measure empty, as the engine shaft here certainly was.

We know that water will always find its own level let what will retard its progress; and why the water here should so suddenly rise in one place and not in another, as it did not in the engine shaft, where there is no resistance, I wish to have properly elucidated.

I shall now state a similar instance near this town, and then briefly state what I conceive to be the cause, and on which point I want to be set perfectly right.

The LOE POOL, which is only separated from the sea by an embankment of small pebbles, commonly called shingles, rises and falls according to the heavy rains, &c. During this last winter, it rose to so great a height, as to require the embankment to be broke, because it impeded the passage of travellers, &c.; but while at its height, I witnessed a large spring, which rose like a fountain, in the centre of a dwelling-house on the side of a hill, situated 150 feet above the surface of the water in the river, and from which there is a gradual descent to the water's edge. When the water again subsided every place was perfectly dry, and a well which is near the spot contained no water, which was a few hours before overflowing.

I here conceive that the outlets were just sufficient to carry off the quantity of water which accumulated; and when the water in the river rose so as to cover these outlets, it made such a resistance, in proportion to the height to which it rose, that the accumulating water or springs could not overcome, consequently it rose to the next outlet, while the original ones still carried off a great quantity.

If this idea is correct, it will equally apply to the case at the mine, but on which, many (to whom I have communicated my thoughts) do not feel perfectly satisfied; and it is for their satisfaction, as well as my own, that I request an answer to explain the cause on proper hydrostatic principles.

I am, Sir,
Your most obedient,
Helston, March 7, 1814. M. P. MOYLE.

III. *On the Ventilation of Mines.**

24th March, 1814.

"The perusal of Mr. John Taylor's valuable paper on the ventilation of coal mines, in your Number for the present month, has brought to my recollection a contrivance which resembles his apparatus in acting as an exhauster of foul air, and which, though of much inferior power, may perhaps be sometimes useful where there is a command of water. This apparatus had been employed at the *Leadhills* in Scotland, and was first mentioned to me by my friend Dr. Stokes of Trinity College, Dublin, whose description I afterwards found, on visiting the mines at that place, to be perfectly correct. It was denominated, from its peculiar mode of action, the *water-sucking-blast*, and was said to have been an imitation of a similar engine employed in Galloway; and though not actually in use when I was at Leadhills in 1809, the works at that time being otherwise sufficiently supplied with air, its construction was remembered very accurately by some of the mine-agents with whom I conversed.

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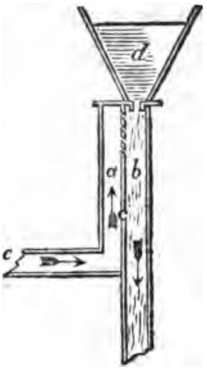
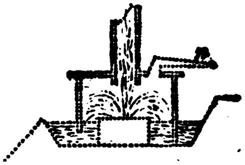
* For this valuable notice the Editor is indebted to Dr. Fitton of Northampton.





" *a* and *b* are square vertical tubes, formed of wood, and separated by a partition *c*, which at the upper part is perforated with a number of oblique openings inclined from *a* towards *b*. *d* is a funnel through which a current of water is admitted into the tube *b*, (by a throat of smaller dimensions than the tube,) and thus conducted to any required depth;—and from the other tube *a*, a branch *e* is given off, which communicates with the place from whence the air is to be drawn out. The water in its descent from the funnel carries along with it a quantity of air, which is continually supplied in the direction of the darts through the tubes *a* and *e*, and discharged below at the termination of *b*; the funnel being kept always filled to a sufficient height to give considerable velocity to the current which descends from it, and the opening of its throat proportioned accordingly to the stream which feeds the apparatus.

" In one of the engines of this description which was employed in ventilating a level, the throat of the funnel was about three inches in diameter, and the pipes three or four inches in the side; the water fell 13 fathoms in the pipe *b*, and it was supposed by a rough estimation, that the apparatus would act with effect at the distance of about 60 fathoms. This distance is not more than one fourth of the range of Mr. Taylor's engine; but there is some ambiguity in the note that I took at Leadhills upon this point, and the distance is probably greater than I have specified.

" I am not sufficiently acquainted with the publications on the practice of mining, to be enabled to state whether this machine has been described in any of them; but in its mode of action it differs essentially from the engines in common use for ventilation, which operate in general by forcing in fresh air: and its effect, as an exhauster, is such as has been suggested by yourself and Mr. Taylor as the best security against the explosion of inflammable gas. An engine is described in the Philosophical Transactions for 1745, vol. xliii., which, from the account given of it in the Abridgment, (vol. ix. p. 109,) appears to have been employed at *Leadhills* to blow the smelting furnaces, and to convey fresh air into the mines. The contrivance for these purposes may be understood from what has been already mentioned, by referring to the dotted lines in the lower part of the annexed sketch; the water discharging the air which it had carried with it, in a large receiver, from whence it passed out in a continued stream through the pipe *n*: and the transition from that contrivance to the engine above described may be readily conceived; for it is evident that the suction, or demand for air at *e*, would be proportioned to the blast that issued from the orifice at *n*.

"The dimensions of such parts of the apparatus described in the Philosophical Transactions as relate to the present purpose, were the following: height of the funnel 5 feet; diameter of the throat of the funnel 3½ inches; diameter of the bore of the pipe *b* 5¼ inches; length of the pipe *b*, 14, 15, or 16 feet; diameter of the air holes at its upper part (two in number) 1½ inch. An engine of these dimensions is said to have afforded, through a hole at *n*, 1½ inch in diameter, a blast of sufficient power "to smelt ore harder than any in Leadhills."

IV. *Meaning of the French Word Génie.*

I am obliged to an anonymous correspondent, who subscribes himself N. N. for the following explanation of the French word *génie*, which I left untranslated in the Biographical Accounts of Lowitz and Malus because I did not know its correct meaning.

"Give me leave to set you right on the subject of the French word 'génie,' as used in the *Annals of Philosophy*, vol. iii. page 171, and pages 241 and 243.

"The Dictionary of the French Academy, after having given the other significations of the word, adds:

'Génie, est aussi l'art de fortifier, d'attaquer, de defèndre une place, un camp, un poste.'—'Il s'est mis dans le génie.'—'Il est dans le génie depuis trois ans.'—'Le corps du génie, the corps of military engineers.'—'Officier du génie.'—'Ingénieur, an officer of engineers.'—'Major du génie, a major of engineers.'—'Ingénieur militaire.'—'Officier de génie militaire,' is used in contradistinction of 'ingénieur des ponts et chaussés,' i. e. a civil engineer and surveyor."

London, 10th April, 1814.

V. *Ventilation of Coal Mines.*

In consequence of the publication of Mr. Taylor's paper on this subject in a preceding Number of the *Annals of Philosophy*, I have been favoured with a visit from Mr. Wilson, who is the patentee of a new pump seemingly constructed upon very ingenious principles. He suggests the application of this pump to coal mines, and says that it will draw out 4000 gallons of air in a minute.

VI. *Method of destroying the Insect that injures Apple Trees.*

(To Dr. Thomson.)

SIR,

In reply to your correspondent's query of last month respecting the most effectual mode of destroying the aphis on apple trees, I beg leave to acquaint you with the method I have practised for some years with complete success. As soon as the insect makes its appearance, which is generally early in the spring, by exuding a white floculent cotton like substance upon such of the rough knotty surfaces of the bark as have afforded it shelter during the winter, I take the first opportunity of examining my trees, and with a pruning-knife cut away all the dead barks from the parts affected, and then immediately cover the wounds by means of a painter's tool brush, with a kind of paint composed of oil of tar and yellow oker, mixed to the consistence of cream. I also proceed in like manner to cover such other parts as may be likely to harbour the insect, or to be subject to its attack. The effect of this operation is immediate and lasting, for the extremely pungent and penetrating property of the oil of tar (being an essential oil) is such, that it instantly insinuates itself through the cracks and fissures of the bark, and thereby effectually destroys both insect and ova in its most secret recesses, without in the smallest degree injuring the tree, and for some months secures the parts from future attack.

The application may be used at all seasons, and by the addition of a little lamp black may be readily made to correspond in colour with the bark of the tree, so as not to become at all offensive to the eye. It is indeed so convenient a medium of defence against the bad effects both of insects and the weather, that I constantly use it after the knife on all occasions.

Your obedient Servant,
RICHARD KNIGHT.

Clapton, April 20, 1814.

P. S. As the oil of tar is not in general use, it may be desirable to know, that it may be procured of D. Hawkins, oilman, 88, Bishopsgate Street without.

ARTICLE XII.

New Patents.

JAMES JAMESON, Colebrook-terrace, Islington; for certain improvements in the construction of fire-arms, and the locks of fire-arms. March 9, 1814.

DANIEL GOODALL, Burton Latimer, Northampton; for manufacturing of English crapes from silks dyed and coloured, both before and after they are thrown or spun into crape, silk, or silk for the manufacturing of crape, and introducing, weaving, or working into the warp and shute of such crapes, black, white, coloured, and fancy silks, and also black, white, coloured, and fancy cottons and worsteds, and also gold and silver, and every other description of plain or fancy materials. March 12, 1814.

ANDREW COOK, Strand, London; for an invention for the prevention and cure of the dry rot and common decay in timber; and for preserving woollen, linen, and other articles, from mildew. March 12, 1814.

Roger Hasledine, Great Russel-street, London, ironmonger; for a contrivance for folding-screens, adapted to impede the passage of air, smoke, fire, and light, applied to fire-places, grates, stoves, windows, and doors, which he denominates "The improved folding screen." March 12, 1814.

Edward Steers, Inner Temple, London; for a method of rendering the stoppers of bottles, jars, &c. air tight. March 12, 1814.

James Barclay and **William Cuming**, Cambridge; for improved wheels and axletrees for carriages. March 12, 1814.

John Slater, Birmingham, manufacturer of coach springs and patent steam kitchens; for an improvement in a steam boiler, and apparatus for the purpose of washing, steaming, cleaning, and whitening cloathes, cloathing, and cloths, and for warming or heating closets, laundries, and other rooms, by the same. March 12, 1814.

ARTICLE XIII.

Scienific Books in hand, or in the Press.

Mr. Saurey is preparing for publication the Morbid Anatomy of the Brain in Mania and Hydrophobia; with the Pathology of the two Diseases, and Experiments to ascertain the presence of Water in the Ventricles and Pericardium, collected from papers of the late Dr. Andrew Marshell.

Mr. Patrick Syme, author of a Treatise on the Art of Flower Painting, is about to publish Werner's Nomenclature of Colours, with examples selected from objects in the Animal, Vegetable, and Mineral Kingdoms.

Dr. Burnett has in the Press a Practical Account of the Mediterranean Fever; also the History of Fever during 1810 to 1813; and of the Gibraltar and Carthagena Fevers.

I. G. Dalyell, Esq. has in the Press Observations of some interesting Phenomena in Animal Physiology, exhibited by various Species of Planariæ, and illustrated by coloured Figures of living Animals.

Mr. Wardrop is printing a Second Volume of Essays on the Morbid Anatomy of the Human Eye.

Mr. Broughton, of Edinburgh, has in the Press a Synthesis and Analysis of the First Ten Powers of Numbers, forming the Introduction to a New Theory of Numbers.

⁂ Early Communications for this Department of our Journal will be thankfully received.

Article XIV.

METEOROLOGICAL TABLE.

1814.	Wind.	Barometer.			Thermometer.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
3d Mo.										
March 14	N E	30·34	30·22	30·280	36	30	33·0	—		☾
15	N E	30·42	30·34	30·380	37	30	33·5	—		
16	N E	30·42	30·32	30·370	40	29	34·5	—		
17	N E	30·32	30·23	30·275	39	28	33·5	—		
18	N E	30·23	30·06	30·145	37	29	33·0	—		
19	N E	30·06	29·78	29·920	35	30	32·5	—		
20	S E	29·78	29·65	29·715	49	35	42·0	—		
21	S E	29·65	29·60	29·625	45	41	43·0	·12	·38	●
22	S W	29·75	29·60	29·675	55	35	45·0	—	—	
23	S W	29·75	29·73	29·740	55	34	44·5	—	—	
24	S W	29·61	29·59	29·600	48	40	44·0	—	—	
25	W	29·61	29·60	29·605	54	35	44·5	—	—	
26	S W	29·80	29·60	29·700	55	41	48·0	—		
27	W	29·84	29·80	29·820	59	36	47·5	—	—	
28	Var.	29·84	29·50	29·670	59	40	49·5	·25	·10	☾
29	S E	29·75	29·50	29·625	52	35	43·5	—		
30	S W	29·87	29·75	29·810	60	31	45·5	—	·36	
31	S E	29·87	29·60	29·735	54	45	49·5	—	·36	
4th Mo.										
April 1	S W	29·60	29·34	29·470	60	42	51·0	—	15	
2	W	29·37	29·31	29·340	56	41	48·5	—	6	
3	S W	29·58	29·37	29·475	57	32	44·5	—		
4	N W	29·70	29·58	29·640	58	34	46·0	—		○
5	Var.	29·90	29·70	29·800	59	38	48·5	—		
6	W	30·00	29·90	29·950	61	38	49·5	—		
7	S E	30·20	30·00	30·100	64	40	52·0	—		
8	N E	30·20	30·16	30·180	63	32	47·5	—		
9	N	—	—	—	—	—	—	—		
10		30·20	29·93	30·065	63	43	53·0	—		
11	N E	29·93	29·83	29·880	62	36	49·0	·83		
		30·42	29·31	29·842	64	28	44·14	1·20	1·41	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Third Month.—14—19. Dull cloudy weather: the latter a misty day. 20. More clear and springlike, after a misty morning. *Cirrus*, passing to *Cirrocumulus* and *Cirrostratus*. 21. Rainy: the morning opened with *Cirrostratus* lowering. 22—25. Variable springlike sky: large *Cumulus* clouds, inosculating with *Cirrocumulus* and *Cirrostratus*. The *Nimbus* appeared occasionally: the air nearly calm, with little evaporation, the mean temperature considered. 27. a. m. Overcast with *Cirrostratus*: a few drops of rain: p. m. Large *Cirri;* sunshine and clouds. 28. a. m. Fair, with *Cirrus* and other light clouds. 29. Overcast: *Cirrostratus*, with *Cumulus* and large *Cumulostratus* clouds. 30. The evaporation is now considerable. Wind and rain by night. 31. Fine morning: wet forenoon: fair afternoon. Thunder clouds appear.

Fourth Month.—1. Cloudy, a. m. with a few drops: a fine day: the *Cumulus* inosculates with the superior clouds: wind and rain by night. 2. Windy morning: squalls, with rain, p. m. A *Nimbus* on the N.W. horizon, with much wind, at sunset. 3. a. m. Windy: *Cumulus* with *Cirrostratus*: p. m. *Cirrus* only, with a brisk evaporation. 4. Morning overcast with the lighter modifications. 5—11. Fair weather: generally misty mornings, with much dew, and clear days. The roads are become already quite dusty by the brisk evaporation.

RESULTS.

Winds variable.

Barometer:	Greatest height	30·42 inches;
	Least	29·31 inches;
	Mean of the period	29·842 inches.
Thermometer:	Greatest height	64°
	Least	28°
	Mean	44·14°

Evaporation 1·20 inches. Rain 1·41 inches.

The frost may be said to have gone off in the first week of the present period, and the mean temperature having steadily advanced since, the latter week has been seasonably warm.

The *Aurora Borealis*, of late years a very unfrequent visitant in these parts, appeared last night, with no great degree of splendour, but with the usual characteristic marks of this phenomenon. About 11 p. m. when my attention was first called to it, there was a body of white light, in part intercepted by clouds, extending at a moderate elevation from the N. to the N. W. with a short broad streamer rising from each extremity. After this it became an arch, composed of similar vertical masses of fibrous light, which moved along in succession, preserving their polarity and curved arrangement. One large streamer, in particular, went rapidly through nearly the whole length of the arch from W. to E., in which direction the rest chiefly moved. Some of these masses were rather brilliant, and one exhibited colours. After some cessation, and a repetition of this appearance, carried more towards E. and W. the light settled in the N., and grew fainter: in which situation, at midnight, I ceased to observe it.

TOTTENHAM, *Fourth Month*, 18, 1814. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

JUNE, 1814.

Article I.

Biographical Account of M. le Comte Lagrange. By M. le Chevalier Delambre.

(*Concluded from* p. 320.)

M. LAGRANGE took possession of his situation on the 6th of November, 1766. He was well received by the king; but soon perceived that the Germans do not like to see foreigners occupy situations in their country. He applied to the study of their language. He devoted himself entirely to mathematics, and did not find himself in the way of any person, because he demanded nothing; and he soon obliged the Germans to give him their esteem. "The king," said he himself, "treated me well, I thought that he preferred me to Euler who was something of a devotee, while I took no part in the disputes about worship; and did not contradict the opinion of any one." This prudent reserve, if it deprived him of the advantages of an honourable familiarity, which would have been attended with some inconveniences, left him the whole of his time for mathematical labours, which hitherto had brought him nothing but compliments the most flattering and the most unanimous. This concert of praises was only once interrupted during the whole of his life.

A French mathematician, who to much sagacity united a still greater degree of selfishness, and scarcely gave himself the trouble to study the works of others, accused M. Lagrange of *having gone astray in the new route that he had traced, from not having well understood the theory of it.* He reproached him with *having deceived himself in his assertions and calculations.* Lagrange in reply expresses some astonishment at these harsh expressions, to which he was so little accustomed. He expected at least to have seen them founded upon some reasons either good or bad; but he discovered nothing of the kind. He shows that the solution proposed by Fontaine was incomplete and illusory in certain respects. Fontaine had boasted that he had taught mathematicians the conditions which render possible the integration of differential equations with three variables. Lagrange showed him by several citations, that these conditions were known to mathematicians long before Fontaine was capable of teaching them. He does not deny that Fontaine discovered these theorems himself, "at least I am persuaded," says he, "that he was as capable of finding them as any person whatever."

It was with this delicacy and moderation that he answered the aggressor. Condorcet, in his *eloge* of Fontaine, is obliged to avow that, on this occasion, his friend deviated from that politeness which ought never to be dispensed with, but which perhaps he thought less necessary with illustrious adversaries, whose glory did not stand in need of these little delicacies. Every one can estimate the value of that apology, especially when applied to a man who, by his own acknowledgment, studied the vanity of others that he might wound it upon occasion. We must at least acknowledge that he, who saw himself attacked in that manner when he was in the right, and who knew how to maintain politeness with an adversary who had himself dispensed with it, acquired a double advantage over him, besides victoriously repelling his imprudent attack.

It will not be expected that we should follow M. Lagrange in the important researches with which he filled the Berlin Memoires; and even some volumes of the Memoirs of the Turin Academy, which was indebted to him for its existence. All the space that can be devoted to this biographical account would not be sufficient even to give an imperfect idea of the immense series of his labours, which have given so much value to the Memoirs of the Berlin Academy, while it had the inestimable advantage of being directed by M. Lagrange. Some of these Memoirs are of such extent and importance that they might pass for a great separate work, yet they constitute a part only of what these twenty years enabled him to produce. He had composed his Mecanique Analytique, but he wanted to have it printed at Paris, where he expected that his formulas would be given with more care and fidelity. On the other hand, it was running too great a risk to intrust the manuscript into the hands of a traveller, who might not be aware of the whole of its value. M. Lagrange made a copy of it, which M. Duchatelet undertook to deliver to the Abbé Marie, with whom he was intimately connected. Marie fulfilled with honour the confidence placed in him. His first care was to find a bookseller who would undertake to publish it; and, what it will be difficult to believe at this time, he could not find one. The newer the methods in it were, and the more sublime the theory, the fewer readers would be found capable of appreciating it; hence, without entertaining any doubts of the merit of the work, the booksellers were excusable in hesitating to print a book, the sale of which would probably be confined to a small number of mathematicians disseminated through Europe. Desain, who was the most enterprizing of all those to whom application was made, would not undertake to publish it, till Marie entered into a formal engagement to take all the copies of the edition which were not sold by a given time. To this first service Marie added another, of which M. Lagrange was not less sensible; he procured him an editor worthy of superintending the publication of such a work. M. Legendre devoted the whole of his time to the troublesome task of correcting the press, and was repaid by the sentiment of veneration for the author with which he was penetrated; and by the thanks which he received from him in a letter which I have had in my possession, and which M. Lagrange had filled with expressions of his esteem and his gratitude.

The book was not yet published when the author came to settle in Paris. Several causes determined him to take this step; but we must not give credit to all that have been stated. The death of Frederick had occasioned great changes in Prussia, and still greater were to be apprehended. Philosophers were no longer so much respected as formerly. It was natural for M. Lagrange again to feel that desire which had formerly conducted him to Paris. These causes, together with the publication of the Mecanique Analytique, were sufficient. It is not necessary to add other causes, which several publications that made their appearance in Germany, and particularly the anonymous historian of the court of Berlin, have noticed. We never, during a residence of 25 years in France, heard M. Lagrange prefer the slightest complaint against the minister, who is accused in that publication of having disgusted him by a treatment full of haughtiness and contempt, which out of respect for himself it was impossible for M. Lagrange to overlook. We might suspect that M. Lagrange had sufficient generosity to forget or pardon bad treatment, which he punished in the only way worthy of himself, by leaving the country where his merit was overlooked; but when he was directly questioned on that subject by a Member of the Institute (M. Burckhardt) he only gave negative answers, and assigned no other motives than the misfortunes which it was thought were about to fall upon Prussia. M. de Hertzberg was dead, and M. de Lagrange, a senator and comte of the French empire, could have no interest in concealing the truth. Hence we must consider his own statement as affording the only true reasons.

The historian therefore, whom we have quoted, has been ill-informed. But the spirit of calumny and satire, which has so justly rendered his work suspected, ought not to prevent us from extracting from it the lines in which he explains, with that energy which is peculiarly his own, his opinion, which is that of all Europe, when he does justice to M. Lagrange.

"I think," says he, (Hist. Secrette de la Cour de Berlin, 1789, tome ii. page 173), "that there is at this moment an acquisition worthy of the king of France, the illustrious Lagrange, the greatest mathematician who has appeared since Newton, and who in every point of view is the man that has the most astonished me;—Lagrange, the wisest, and perhaps the only practical philosopher that ever existed, meritorious by his undisturbable wisdom, his manners, his conduct; the object of the most tender respect of the small number of men with whom he associates;—Lagrange is misunderstood; every thing leads him to leave a country where nothing can excuse the crime of being a foreigner, and where in fact he is merely tolerated. Prince Cardito de Laffredo, Neapolitan minister at Copenhagen, offered him the most flattering conditions on the part of his sovereign. The Grand Duke, the King of Sardinia, invite him eagerly; but all their proposals would be easily obliterated by ours. I am very eager to see this proposal made, because I consider it as noble, and because I tenderly love the man who is the object of it. I have induced M. Lagrange not to accept immediately the proposals made to him, and to wait till he receives ours."

The author whom we quote appears to fear the opposition of M. Breteuil; but, according to M. Lagrange himself, it was the Abbé Marie who proposed it to M. Breteuil, who on all occasions anticipated the desires of the Academy of Sciences, presented the demand to Louis XVI, and induced him to agree to it.

The successor of Frederick, although he did not much interest himself in the sciences, made some difficulty in allowing a philosopher to depart whom his predecessor had invited, and whom he honoured with his particular esteem. After some delay, M. Lagrange obtained liberty to depart. It was stipulated that he should still give some memoirs to the Berlin Academy. The volumes of 1792, 1793, and 1803, show that he faithfully kept his promise.

It was in 1787 that M. Lagrange came to Paris to take his seat in the Academy of Sciences, of which he had been a foreign member for 15 years. To give him the right of voting in all their deliberations, this title was changed into that of *veteran pensionary*. His new associates showed themselves happy and proud in possessing him. The Queen treated him with regard, and considered him as a German. He had been recommended to her from Vienna. He obtained a lodging in the Louvre, where he lived happy till the Revolution.

The satisfaction which he enjoyed did not show itself outwardly. Always affable and kind when interrogated, he himself spoke but little, and appeared absent and melancholy. Often in companies which must have been suitable to his taste, among the most distinguished men of all countries who met at the house of the illustrious Lavoisier, I have seen him dreaming, as it were, with his head against a window, where however nothing attracted his attention. He remained a stranger to what was passing around him. He acknowledged himself that his enthusiasm was gone, that he had lost his taste for mathematics. When informed that a mathematician was employed at such a task, "so much the better," he would say, "I had begun it, now it will be unnecessary for me to finish it." But he merely changed the object of his studies. Metaphysics, the history of human nature, that of different religions, the general theory of languages, medicine, botany, divided his leisure hours. When the conversation turned upon subjects with which it was supposed he was unacquainted, we were struck by an unexpected observation, a fine thought, a profound view, which excited long reflections. Surrounded by chemists who were reforming the theory and even the language of the science, he made himself acquainted with their discoveries, which gave to facts formerly isolated that connection which distinguishes the different parts of mathematics. He undertook to make himself acquainted with this branch of knowledge, which formerly appeared to him so obscure, but which he found on trial *as easy as algebra.* People have been surprized at this comparison, and have thought that it could come from no one else than Lagrange. It appears to us as simple as just; but it must be taken in its true sense. Algebra, which presents so many insoluble problems, so many difficulties against which Lagrange himself struggled in vain, could not in that sense appear to him an easy study. But he compares the new elements of chemistry with those of algebra. They constituted a body, they were intelligible, they offered more certainty, they resembled algebra, which in the part of it that is complete presents nothing difficult to conceive, no truth to which we may not arrive by the most palpable reasoning. The commencement of the science of chemistry appeared to him to offer the same advantages, perhaps with somewhat less stability and certainty; but, like algebra, it has no doubt also its difficulties, its paradoxes, which will require, to explain them, much sagacity, reflection, and time. It has likewise its problems which never will be resolved.

In this philosophic repose he continued till the Revolution, without adding any thing to his mathematical discoveries, or even opening his Mecanique Analytique, which had been published for two years.

The Revolution gave philosophers an opportunity of making a great and difficult innovation: the establishment of a system of weights and measures founded on nature, and perfectly analogous to our scale of numbers. Lagrange was one of the commissioners whom the Academy charged with that task. He was one of its keenest promoters. He wished to see the decimal system in all its purity. He was provoked at the complaisance of Borda, who got quarters of the *metre* made. He thought the objection of little importance which was drawn against the system from the small number of divisors that its base afforded. He regretted that it was not a prime number, as 11, which would have given the same denominator to all the fractions. This idea perhaps will be regarded as one of those exaggerations, which are hazarded by men of the best understandings, in the heat of dispute. But he mentioned the number 11 merely to get rid of the number 12, which more intrepid innovators would have wished to substitute in place of the number 10, that constitutes the base of the whole of our numeration.

When the Academy was suppressed, the commission charged with the establishment of the new system was retained for a time. Three months had scarcely elapsed when, in order to *purify* that commission, the names of Lavoisier, Borda, Laplace, Coulomb, Brisson, and Delambre, were struck out. Lagrange was retained. In quality of President, he informed me, in a long letter full of kindness, that I should receive official information of my removal. As soon as he saw me on my return to Paris, he expressed to me his regret at the dismission of so many associates. "I do not know," said he, "why they have retained me." But unless the suppression had been total, it could scarcely have extended to him. The more losses the commission had sustained, of the more importance was it not to deprive it of the consideration attached to the name of Lagrange. Besides, he was known to be wholly devoted to the sciences; he had no place either in the civil department or the administration. The moderation of his character had prevented him from expressing what he could not but think in secret: but I shall never forget the conversation which I had with him at that period. It was the day after the atrocious and absurd sentence, contrary to every thing like justice, had thrown all lovers of the sciences into mourning, by cutting off the most illustrious philosopher in Europe. "It has cost them but a moment," said he, "to cut off that head, and a hundred years perhaps will not be sufficient to produce another like it." Some months before we had had a similar conversation in the cabinet of Lavoisier, on account of the death of the unfortunate Bailly. We lamented together the dreadful consequences of the dangerous experiment which the French had attempted. All these chimerical projects of amelioration appeared to him very equivocal proofs of the greatness of the human mind. "If you wish to see it truly great," added he, "enter into the cabinet of Newton employed in decomposing light, or in explaining the system of the world."

Already for some time he had regretted not having listened to the advice of his friends, who at the commencement of our troubles had recommended him to seek an asylum, which it would have been so easy for him to find. As long as the revolution seemed only to threaten the pension which he enjoyed in France, he had neglected that consideration, out of curiosity to be upon the spot of one of those great convulsions which it is always more prudent to observe at a distance. "It was your own choice," said he several times to himself when he entrusted me with his regret. It was to no purpose that a special decree of the Constituent Assembly had ensured the payment of his pension. The decree was of no value, because the depreciation of the paper currency was sufficient to render it illusory. He had been named member of the *Board of Consultation*, appointed to examine and reward useful inventions. He had been appointed one of the administrators of the Mint. This commission offered him few objects to fix his attention, and could in no degree remove his apprehensions. It was again proposed to draw him to Berlin, and to restore him to his former situation. He had agreed to the proposal. Herault de Sechelles, to whom he had applied for a passport, offered him, for the greater security, a mission to Prussia. Madame Lagrange would not consent to quit her country. This repugnance, which at that time he considered as a misfortune, was to him a source of fortune and of new glory.

The Normal School, of which he was named Professor, but which had only an ephemeral existence, scarcely gave him time to explain his ideas respecting the foundation of arithmetic and algebra, and their application to geometry.

The Polytechnic School, the result of a happier idea, had likewise a more durable success: and among the best effects which it produced, we may place that of having restored Lagrange to Analysis. It was there that he had an opportunity of developing those ideas, the germ of which was to be found in two memoirs that he had published in 1772, and the object of which was to explain the true metaphysics of the differential and integral calculus. To render these happy developements more easily understood, the professor associated himself with his pupils. It was then that he composed his Analytical Functions, and his Lectures on that Calculus, of which he published several editions. "Those who have it in their power to attend these interesting lectures," said publicly one of the Professors (M. Lacroix), "have the pleasure to see him create before the eyes of his audience almost the whole of his theory, and will carefully preserve several variations which the historian of the science will collect as examples of the path followed in analysis by the genius of invention.

It was then likewise that he published his treatise on the numerical solution of equations, with notes on several points of the theory of algebraic equations.

It is said that Archimedes, whose great reputation, at least with the historians, is founded upon the machines of all kinds, by means of which he retarded the taking of Syracuse, despised these mechanical inventions, on which he wrote nothing, and placed importance only in his works of pure theory. We may sometimes conceive that the great mathematicians of our age entertained the same sentiments with Archimedes. They consider a problem as solved when it presents no analytical difficulty, when nothing remains to be done but differentiations, substitutions, and reductions, operations which require merely patience, and a certain dexterity derived from practice. Satisfied with having removed all the real difficulties, they concern themselves perhaps too little with the embarrassments which they leave to the calculator, and with the long labour necessary in order to make use of their formula, even after it has been suitably reduced. M. Lagrange had more than once attempted to abridge the usual calculations.

The general resolution of algebraic equations is subject to difficulties which are considered as insurmountable; but in practice every determinate problem brings us to an equation, all the coefficients of which are given in numbers. It would be sufficient therefore to have a sure method of finding all the roots of such an equation, which is called *numerical.* This was the object which M. Lagrange proposed to himself. He analyses all the known methods, and shows their uncertainty and insufficiency. He reduces the problem to the determination of a quantity smaller than the smallest difference between the roots. This is something. We cannot too much admire the analytical skill displayed throughout the whole work. But notwithstanding all the resources of the genius of M. Lagrange, we cannot conceal that the labour of his method is exceedingly great, and calculators will doubtless continue to prefer methods less direct indeed, but more expeditious. The author resumed this subject no less than four times. It is to be feared that a commodious and general solution will never be discovered, or at least it must be sought for by other means. The author seems to have acknowledged this himself, as he recommends the method of M. Budan as the most convenient and elegant for resolving equations whose roots are all real.

The desire of multiplying useful applications induced him to undertake a new edition of the Mecanique Analytique. His project was to develope the most useful parts of it. He laboured at it with all the ardour and intellectual power which he could have applied at any period of his life. But this application occasioned a degree of fatigue which threw him into a fainting fit. He was found in that state by Madame Lagrange. His head in falling had struck against the corner of a table, and this shock had not restored him to his senses. This was a warning to take more care of himself. He thought so at first; but he was too anxious to finish his work, the printing of which is at present at the 26th sheet of the second volume. The first volume had appeared some time before his death. It had been followed by a new edition of his Fonctions Analytiques. So much labour exhausted him. Towards the end of March a fever came on, he lost his appetite, his sleep was uneasy, and his waking was accompanied by alarming swoonings. He perceived his danger; but, preserving his undisturbable serenity, he studied what passed within him, and, as if he were assisting at a great and uncommon experiment, he bestowed all his attention on it. His remarks have not been lost. Friendship conducted to his house on the 8th of April, in the morning, MM. Lacepede, Monge, and Chaptal, who took care to write down the principal points of a conversation which was his last. (We have scrupulously followed these notes, and the passages under inverted commas are faithfully copied from the manuscript of M. Chaptal.)

"He received them with tenderness and cordiality. I was very ill, my friends (said he), the day before yesterday; I perceived myself dying, my body became weaker, my moral and physical powers were gradually declining; I observed with pleasure the gradual diminution of my strength, and I arrived at the point without pain, without regret, and by a very gentle declivity. Death is not to be feared, and when it comes without violence it is a last function which is neither painful nor disagreeable." Then he explained to them his ideas respecting life, the seat of which he considered as spread over the whole body, in every organ and all parts of the machine, which in his case became equably feebler in every part by the same degrees. "A little longer, and there would have been no functions, death would have overspread the whole body, for death is merely the absolute repose of the body; I wished to die," added he with greater force, "I found a pleasure in it; but my wife did not wish it. I should have preferred at that time a wife less kind, less eager to restore my strength, and who would have allowed me gently to have finished my career. I have performed my task, I have acquired some celebrity in the mathematics, I have hated nobody, I have done no ill; it is now proper to finish."

As he was very animated, especially at these last words, his friends, notwithstanding the interest with which they listened to him, proposed to retire. He retained them, began to relate to them the history of his life, of his labours, of his success, of his residence at Berlin, where he had often told us what he had seen near a King; of his arrival at Paris, the tranquillity he had enjoyed at first, the anxiety occasioned to him by the Revolution, and how he had been finally rewarded by a powerful monarch, capable of appreciating his worth, who had loaded him with honours and dignities, and who had even lately sent him the Grand Ribbon of the Imperial Order of Re-union. Let us add likewise, who after having given him during his life the most unequivocal proofs of the highest esteem, has since done more for his widow and his brother than ever Frederick had done for him while he was Director of his Academy.

He had neither been ambitious of riches nor honour; but he had received both with respectful gratitude, and rejoiced at the acquisition for the advantage of the sciences. He meant to affix these titles to the frontispiece of his work, "in order to show the universe to what a degree the Emperor loved and honoured philosophers."

From these last words we see that he had not lost all hope of cure; he believed only that his convalescence would be long. He offered, when he recovered his strength, to go and dine at M. Lacepede's country house with MM. Monge and Chaptal, and proposed to give them details respecting his life which could be found nowhere else. These details are irretrievably lost. We do not even know to what he alluded, nor what he could have added to the second volume of the Mecanique Analytique, which was then in thepress. We have just learned that the Countess Lagrange has put into the hands of M. Prony the complete manuscript of the second volume, in which will be found important additions, and sections entirely written anew. By the care of an editor so skilful, and so devoted to the memory of the author, the philosophical world is sure of obtaining with the greatest accuracy and dispatch what is wanting to complete the work, and perhaps even memoirs entirely new.

"During this conversation, which lasted more than two hours, his memory often failed him; he made vain efforts to recover names and dates, but his discourse was always connected, full of strong thoughts and bold expressions." This exercise of his faculties wasted the whole remains of his strength. Scarcely had his friends left him, when he fell into a fainting fit, and he died two days after, on the 10th of April, 1813, at three quarters past nine o'clock in the morning.

M. Lagrange was of a delicate but good complexion. His tranquillity, his moderation, an austere and frugal regimen, from which he rarely deviated, prolonged his life to the age of 77 years, two months, and ten days. He was twice married: first at Berlin, in order to be on a footing with the rest of the academicians, none of whom were bachelors. He brought from Turin one of his relations. He married her, and lost her after a long illness, during which he had bestowed on her the most tender and unremitted care. When he afterwards married, in France, Mademoiselle Lemonnier, daughter of the celebrated astronomer of that name, he said to us, "I had no children by my first marriage; I do not know if I shall have them by my second; but I scarcely desire them." What he principally wished was an amiable companion, whose society might afford him some amusement during the intervals of his studies, and in this respect he was very successful. Madame Lagrange, daughter, grandaughter, and niece, of members of the Academy of Sciences, was deserving of the name which he gave her. This advantage in her eyes making up for the difference of their ages, she soon felt for him the tenderest regard. He was so grateful that he could scarcely bear to be separated from her, and it was on her account alone that he felt any regret at relinquishing this life; and he was often heard to say, that of all his good fortune, that which he prized the most was having obtained a companion so tender and attached to him. During the ten days that his illness lasted she never quitted him for a moment, and was constantly employed in recruiting his strength and prolonging his existence.

He loved retirement; but did not insist upon his young wife following his example. On her account he went out more frequently, and indeed his high situations obliged him to show himself in the world. It was often apparent that he continued the meditations in public which he had begun in his cabinet. It has been said that he was not insensible to the charms of music. In fact, in a numerous company he was not displeased at a concert. On one of these occasions I asked him what he thought of the music: "I love it," says he, "because it leaves me to myself. I listen to it during the first three measures, but I hear no more of it; I give myself up to reflection, nothing interrupts me, and in this way I have solved many a difficult problem." Hence the finest music must have been that during which he was inspired with the finest of his thoughts.

Though he had a venerable figure, indicating his excellent characteristics, he would never allow his portrait to be drawn. More than once, by a very excusable piece of address, persons have been introduced during the meeting of the Institute to take a sketch of him without his knowledge. An artist sent by the Academy of Turin drew in this manner the outline from which was constructed the bust that was exhibited for some months in the hall of the Institute, and is at present in the library. A cast was taken of him after his death; and some time before, while he slept, a picture of him was taken, which is said to resemble him very much.

Gentle and even timid in conversation, he took a pleasure in asking questions, either to draw out others, or to add their reflections to his own vast knowledge. When he spoke, it was always in a tone of doubt, and his first words usually were, *I do not know*. He respected the opinions of others, and was very far from laying down his own as a rule. Yet it was not easy to make him change them. Sometimes he even defended them with a degree of heat which continued to increase till he was sensible of some alteration in himself; then he immediately resumed his usual tranquillity. One day, after a discussion of this kind, M. Lagrange having left the room, Borda remaining alone with me, allowed these words to escape him: "I am sorry to say it of a man like M. Lagrange, but I do not know a more obstinate person." If Borda had gone away first, Lagrange might have said to me as much of our associate, who was a man of excellent sense and considerable wit; but who, like Lagrange, did not easily abandon those opinions which he had adopted after a mature examination.

A gentle and good-natured irony was often remarkable in the tone of his voice; but I never saw any person hurt at it; because it was necessary to have well understood every thing that went before to perceive the true intention of it.

Among all the master-pieces which we owe to his genius, his Mecanique is certainly the most remarkable and the most important. The Fonctions Analytiques hold only the second place, notwithstanding the fruitfulness of the principal idea, and the beauty of the developements. A notation less commodious, and calculations more embarrassing, though more luminous, will prevent mathematicians from employing, except in certain difficult and doubtful cases, his symbols and names. It is sufficient that he has proved the legitimacy of the more expeditious processes of the differential and integral calculus. He has himself followed the ordinary notation in the second edition of his Mecanique.

This great work is entirely founded on the calculus of variations, of which he was the inventor. The whole flows from a single formula, and from a principle known before his time; but the whole utility of which was far from suspected. This sublime composition includes all his other preceding labours which could be connected with it. It is distinguished likewise by the philosophical spirit which reigns from one end of it to the other. It is likewise the best history of that part of the science, a history which could only have been written by a man perfectly master of his subject, and superior to all his predecessors, whose works he analyses. It forms a most interesting piece of reading even to him who is not capable of appreciating all the details. Such a reader will at least find the intimate connection of all the principles on which the greatest mathematicians have founded their researches into mechanics. He will there see the geometrical law of the celestial motions deduced from simple mechanical and analytical considerations. From those problems, which serve to calculate the true system of the world, the author passes to questions more difficult, more complicated, and which belong to another order of things. These researches are only objects of pure curiosity, as the author announces, but they show the extent of his resources. Finally, we see there his new theory of the variations of arbitrary constant quantities of the motion of the planets, which had appeared with so much eclat in the Memoirs of the Institute, where it had shown that the author, at the age of 75, had not sunk from the rank which he had filled for so long a time in the opinion of all mathematicians.

In every part of his writings, when he makes use of an important theorem, he names the original discoverer of it.

When he opposes the ideas of his predecessors and contemporaries, it is, with all the attention due to genius: when he points out the errors of those who have attacked him, it is with the apathy of a true mathematician, and the calmness of a demonstration. None of his celebrated rivals had ideas more just, more fine, more general, and more profound. Finally, thanks to his happy labours, the science of mathematics is now like a vast and fine palace, the foundations of which he renewed, and in which we cannot take a single step without perceiving admirable monuments of his genius.

ARTICLE II.

Contributions to the Chemical Knowledge of Manganese.[*]

By Dr. John.

(*Continued from* vol. ii. p. 271.)

ACTION OF NITRIC ACID ON MANGANESE.

a. On the Metal.

Nitric acid when moderately concentrated, readily dissolves manganese with the evolution of considerable heat, and the escape of nitrous gas. The solution is colourless. It exhibits the same properties as the solution of the white oxide in the same acid, of which I shall speak immediately.

By long continued evaporation the nitric acid is completely decomposed; nitrous gas makes its escape, and the manganese remains in the state of a black oxide. This experiment enabled me to determine the quantity of oxygen present in the black oxide of manganese.

b. On the imperfect Oxide.

Both the green oxide and the white carbonate dissolve with great facility in nitric acid. Of all the crystallizable salts of manganese, this is the most difficult to bring to the state of regular crystals. Most chemists indeed doubt the possibility of obtaining such crystals; but I have been fortunate enough to succeed in the following manner. I evaporated a neutral solution of this salt in a porcelain vessel as far as possible, without decomposing the acid, and allowed it to cool rapidly. The whole solution concreted into a solid mass. I mixed it with a very little water, heated it rapidly, then covered up the vessel, and suffered it to cool at the temperature of 59° Fahrenheit. After an interval of some days, I found crystals at the bottom of this solution, which possessed the following properties.

They have the form of prismatic needles, which running parallel to the bottom of the vessel, stretch from the one side to the other. The faces of the crystals are channelled lengthwise. They have a white colour, are semi-transparent, and have a sharp bitterish taste.

When exposed to the air, they deliquesce still more speedily than muriate of manganese. They cannot endure a high temperature; but melt in the twinkling of an eye, and are completely decomposed.

They dissolve in alcohol, and the solution gives to combustible bodies dipt in it, the property of burning with a green coloured flame.

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[*] Translated from Gehlen's Journal fur die Chemie und Physik. Vierter band. Sid. 436. Not having been able to find room for so long a time for the insertion of this paper, we have thought it better to delay some of our other papers than defer it longer.—T.

In consequence of the great solubility of these crystals, it is difficult to determine their specific gravity; but it does not seem to differ much from that of muriate of manganese.

The solution of these crystals is decomposed by the alkaline oxalates and phosphates.

ACTION OF BENZOIC ACID.

a. On the Metal.

Benzoic acid acts very slowly upon metallic manganese. If they be digested together in water for some hours, the water is decomposed, and the metal is converted into a light green oxide, which gradually dissolves in the acid. The solution possesses the same properties as the following.

b. On the green Oxide and white Carbonate.

Both are very slowly dissolved in benzoic acid by the assistance of heat, the last with a very feeble effervescence. The solutions are colourless and readily crystallize.

Properties of the Crystals.

Benzoate of manganese by slow evaporation crystallizes in long thin prisms; but when we evaporate rapidly we usually obtain irregular plates.

The crystals are colourless, transparent, not altered by exposure to the air. The taste is at first sweet and somewhat astringent, but it becomes at last bitterish.

At the temperature of 66° they dissolve in 20 times their weight of water. They are much more soluble in boiling water, and the solution crystallizes as it cools. They are soluble likewise in alcohol. The solution is decomposed by the alkaline prussiates, carbonates, phosphates, and tungstates.

100 grains of this salt distilled in a retort till they were decomposed gave only some drops of water, but furnished a considerable quantity of oil. This oil had the colour, consistence, and agreeable taste of oil of cinnamon, which at first was mixed with the taste of prussic acid, though none of that acid could be detected by the most careful examination. The residuum in the retort after being fully charred, was dissolved in muriatic acid, and precipitated by carbonate of potash. The oxide obtained weighed 24 parts. Hence 100 grains of benzoate of manganese consist of

Green oxide	24
Acid and water	76
	100

ACTION OF SUCCINIC ACID.

a. On the Metal.

Manganese dissolves very rapidly in succinic acid. During the solution hydrogen gas is evolved, which has the smell of asafœtida. The solution has at first a greenish colour, owing to the quantity of green oxide formed. When it is nearly saturated it becomes pale red. A small quantity of charcoal, previously contained in the metal, remains behind undissolved, as happens in all solutions of this metal.

b. On the green Oxide.

This acid dissolves the green oxide quietly, and the carbonate with strong effervescence. The solution has a faint red colour, and crystallizes very readily.

Properties of the crystallized Salt.

The crystals have the following shapes: 1. A somewhat oblique, four-sided prism. 2. A double four-sided pyramid, having the alternate edges of the common basis truncated. 3. An equal four-sided table, with flat ends, and two of the sides narrower than the other two. These two narrow sides and the edges of the table are often truncated.

The crystals are usually transparent. Taken separately they appear colourless; but when viewed in considerable groups, they have a weak red colour, and a very strong lustre. Their taste is acid and salt. They are not altered by exposure to the air. When heated they become opake and white, and assume the appearance of porcelain.

At the temperature of 66°, they dissolve in ten times their weight of water. They are insoluble in alcohol.

100 grains of this salt were distilled in a small pneumatic apparatus. There came over, first, water, then a yellowish grey smoke, and lastly a brown oil. During this time a considerable quantity of gas was disengaged, which was at first pure carbonic acid, and afterwards a mixture of carbonic acid and carbureted hydrogen. The residue freed from the remains of the succinic acid, consisted of 30·27 of green oxide. Hence 100 parts of succinate of manganese consist of

Green oxide	30·27
Acid and water	69·37
	100·00

ACTION OF ACETIC ACID.

a. On the Metal.

Acetic acid acts upon manganese very slowly, but completely dissolves it at last. The solution has a reddish colour and readily crystallizes.

b. On the green Oxide.

Concentrated acetic acid dissolves both the green oxide and the carbonate of manganese. In order to saturate the acid completely, it must be digested for some days on the oxide.

Properties of the crystallized Salt.

The shape of the crystals is a rhomboidal table, having commonly two of its opposite angles truncated. They have a red colour, are transparent, are not altered by exposure to the air, and have a peculiar but weak metallic taste, with a certain degree of astringency. They are soluble both in water and in alcohol. At the common temperature they require about $3\frac{1}{8}$ times their weight of water to dissolve them.

When heated they exhibit the same phenomena as succinate of manganese. The solution of this salt in water is decomposed by the alkaline carbonates, prussiates, molybdates, and arseniates, but not by the alkaline borates and oxalates. Arsenic acid produces a precipitate of arseniate of manganese.

This salt analysed in the same way as the preceding salt, was found to consist of

Oxide	30
Acid and water	70
	100

ACTION OF CHROMIC ACID.

a. On the Metal.

Chromic acid acts but slowly on manganese. However, by the application of a moderate heat, water is decomposed, hydrogen gas evolved, and the oxide gradually unites with the acid and is dissolved.

b. On the green Oxide.

Chromic acid dissolves the green oxide with tranquillity, and the carbonate with effervescence. The solution is never completely neutral, but always contains an excess of acid. When concentrated it has a dark chesnut brown colour, and a sharp metallic taste. The chromate of manganese cannot be obtained in a metallic form. When evaporated the manganese absorbs oxygen, and falls in the state of a black powder, united with a small portion of chromic acid. If the solution be repeatedly evaporated, and then diluted with water, the manganese gradually precipitates, and the chromic acid remains united to a very small proportion of the oxide.

The solution of chromate of manganese is decomposed by the alkaline carbonates and prussiates. The precipitate obtained consists of carbonate or prussiate of manganese. If nitrate of silver be dropt into the solution of this salt, a fine black precipitate falls, consisting of chromic acid and oxide of silver, united with some oxide of manganese. Nitrate of manganese remains in solution.

ACTION OF TUNGSTIC ACID.

A mixture of powdered manganese and tungstic acid in hot water, for some weeks, the water was gradually decomposed, and the oxide formed united with the acid, and constituted a white powder. The green oxide and carbonate of manganese unite with tungstic acid nearly as slowly. Tungstate of manganese is most easily obtained by decomposing a solution of manganese, by means of tungstate of potash. The precipitate, thus obtained, is to be collected, washed, and dried.

The tungstate of manganese has a white colour. It is tasteless, insoluble in water, and not altered by exposure to the air. Before the blow pipe upon charcoal, it assumes first a yellow, and then a brown colour; but does not melt.

ACTION OF ARSENIC ACID.

a. On the Metal.

Arsenic acid dissolves the metal, and when the acid is nearly saturated, arseniate of manganese precipitates in the form of a gelly.

b. On the Oxide.

Arsenic acid dissolves both the green oxide and the carbonate. The solution always contains an excess of acid. If we nearly saturate the acid, the solution immediately gelatinizes, and neutral arseniate of manganese precipitates, which is insoluble in water. If some drops of diluted sulphuric acid be dropt into the gelatinous mass, a complete solution is again effected, and by evaporation striated crystals are obtained, which appear to be a triple salt, composed of sulphuric acid, arsenic acid, and oxide of manganese.

ON THE OXIDATION OF MANGANESE.

How difficult it is to determine the proportion of oxygen which unites with a metal in its different degrees of oxidation, is obvious from the different results obtained by the latest and best chemists in their analyses. That this difficult determination must hold in all metals, whose oxides on the one hand, while imperfectly saturated with oxygen, absorb more of that principle by simple exposure to the air, or on the other, containing a maximum of oxygen, give out a portion of that substance when exposed to a high temperature, must be obvious to every one who is in the least acquainted with practical chemistry. These difficulties hold in a very high degree, when we attempt to determine the composition of the oxides of manganese. And though by varying and frequently repeating my experiments, I succeeded in obtaining a constant result, yet I must acknowledge, that nothing more seems to have been established by me, than the limits of the different oxides.

From my trials, it appears that there are three oxides of manganese different from each other, in which the proportion of oxygen may be determined; namely the *green*, the *brown*, and the *black*. Besides these oxides indeed, I thought I observed the metal passing into several others. Thus I thought the green, when deprived of a portion of its oxygen, became greenish grey; and the black oxide in like manner became white: but it is not possible to determine the difference in the quantity of oxygen which these supposed oxides contain.

Manganese is so much inclined to form red solutions in acids, that one might be disposed to suspect that one of its oxides has a red colour; but I have not only not been able to establish the existence of any such oxide, but not even able to obtain any red precipitate from such solutions. Hence I consider the statement contained in different chemical systems of the existence of this red oxide as erroneous. I am not ignorant that nature presents to us red coloured minerals, which if we trust the analyses that have been made of them, owe their red colour to manganese; for example, milk quartz and Siberian red schorl. But it is well known, that when various substances are united together, they modify the colour of each other. It is equally well known, that nothing is easier in such analyses, than to overlook the real colouring matter altogether. Hence I do not consider the existence of these minerals as a proof that manganese is capable of forming a red oxide.

A. Examination of the green Oxide.

a. Oxidation of Manganese by the Decomposition of Water.

Metallic manganese decomposes water at the ordinary temperature of the atmosphere with considerable rapidity. The metal is changed into a greyish green oxide, combining with the oxygen of the water, while the hydrogen makes its escape. I put 80 grains of pure metal into a small vessel, contrived so as to collect the gas, and filled the vessel with distilled water. The gas occupied very nearly the bulk of 24 ounces of water. It was hydrogen gas, but probably held in solution some atoms of the metal; for it had a peculiar smell, and burnt with a green coloured flame. The evolution of the gas continued for a whole day and then stopped; and though the water was heated, the oxide did not appear to undergo any change. It was rapidly dried in a close vessel, and was found to weigh 92 grains. Hence green oxide of manganese is a compound of

Metal	86·97
Oxygen	13·03
	100·00

When this oxide comes in contact with the air, either immediately or through water, it absorbs more oxygen, and is changed into brown oxide.

b. Estimation of the Proportion of Oxygen, the dry way.

It has been formerly shown that, in order to form 100 parts of carbonate of manganese, 48·60 parts of the metal are required, and that 100 grains of this carbonate after being heated to redness in a retort, leave 55·84 grains of green oxide. Hence it follows, that 55·84 of green oxide contain 48·6 of metal, and consequently 7·24 of oxygen. Now 55·84 : 7·24 :: 100 : 12·96. Therefore green oxide is composed of

Metal	87·04
Oxygen	12·96
	100·00

This estimate differs from the preceding only 0·007. We may therefore consider 0·13 as the proportion of oxygen in 1·00 of green oxide.

B. Examination of the brown Oxide.

I exposed the 92 grains of green oxide obtained by the decomposition of water, for some days to the action of the air, until it was completely changed to a dark brown powder. I then heated it for an instant in a close vessel, and then weighed it. The weight was increased 8 grains; so that 80 grains of metal, in order to be changed into brown oxide absorb 20 grains of oxygen. Hence the chesnut brown oxide of manganese is composed of

Metal	80
Oxygen	20
	100

This oxide still continues to absorb oxygen from the atmosphere; but the absorption goes on so slowly, that after an interval of several days the change of weight is hardly perceptible.

I obtained the same result by exposing the pure metal for some days to the open air, and then heating it in a small retort, in order to drive off any moisture which it might have absorbed.

C. Examination of the black Oxide.

Though this oxide has been already examined by several celebrated chemists, yet the difference in their estimates of its composition is so great, that more experiments are obviously necessary to determine the point.

From the following experiment, which succeeded equally on several repetitions, it seems clear that the proportion of oxygen, supposed by several chemists to exist in this oxide, is excessive.

I dissolved 100$\frac{3}{4}$ grains of metallic manganese in nitric acid, with the exception of $\frac{3}{4}$ grain which remained undissolved, and was separated by the filter. The solution was put into a small retort, and cautiously distilled to dryness. I then broke the retort, and collected and weighed the black porous shining oxide which remained behind. Its weight was 140 grains. To see whether it was pure oxide, a portion of it was digested in water, but no nitrate of manganese was dissolved. Another portion was heated to redness in a small retort connected with a pneumatic apparatus. The gas evolved was pure oxygen gas. Hence 100 parts of black oxide of manganese are composed of.

Metal	71·33
Oxygen	28·67
	100·00

Article III.

Mineralogical Observations in Galloway. By James Grierson, M.D. Read before the Wernerian Society, March 6, 1814.

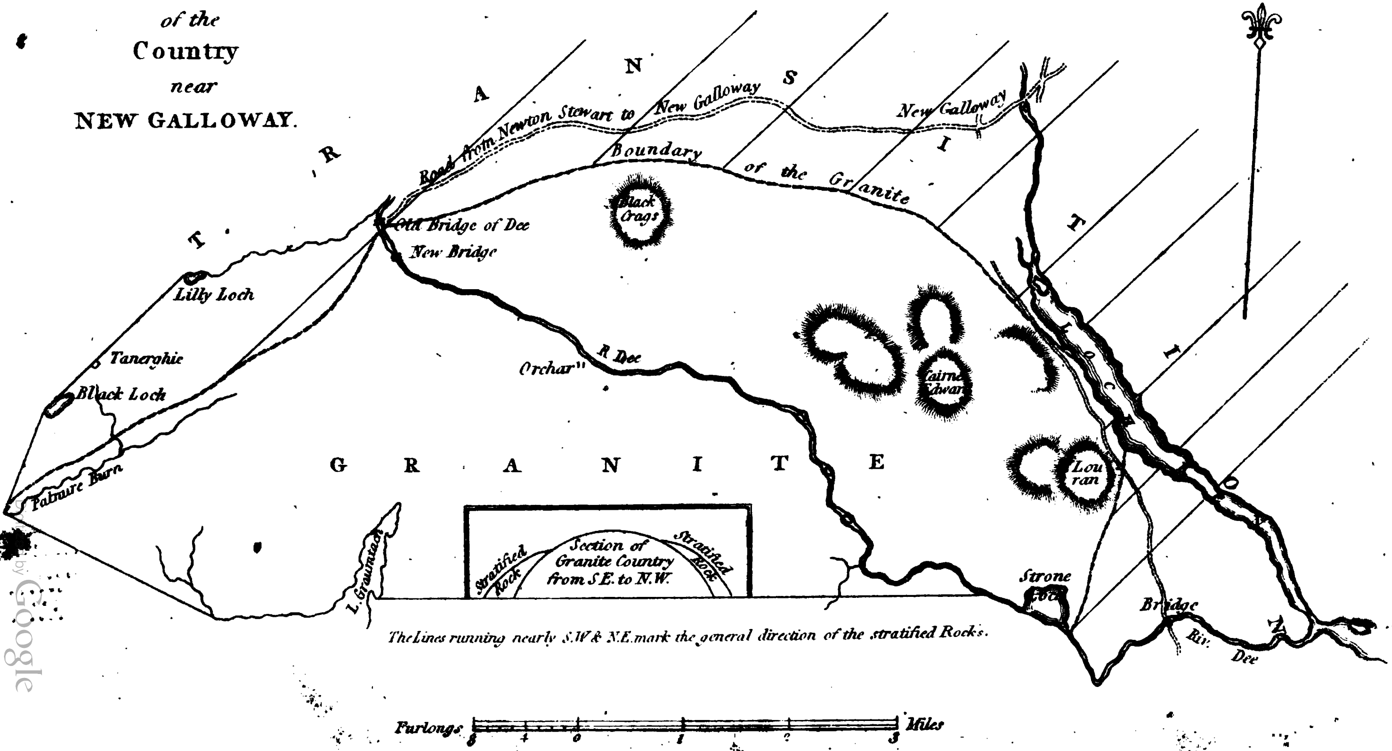
(With a Map.)

It is now I believe known to every mineralogist, of this country at least, that three distinct masses of granite occur in the transition district which principally constitutes the two counties of Galloway: the westernmost, or Loch Doon mass; the middle, or Dee mass; and the easternmost, or Criffle mass. These are nearly of equal sizes, and include each a space of about eight or ten miles by three or four. The Loch Doon and Criffle masses I have not had an opportunity of examining; but the middle, or Dee one, has at different times occupied my attention. The granite in each of these three districts appears to be in no part of its boundaries at a great distance from the transition rocks, viz. greywacke and greywacke slate, of which by far the greater part of the counties of Galloway is composed.

The Dee granite district extends from within about two miles of the borough of New Galloway, on the north, to within nearly the same distance of Creetown, on the south, or to the bay of Wigton, at the mouth of the river Cree. It includes the following hills, which rise to a considerable height: Blackcraig, Cairn Edward, Louran, Hill of Orchar, Hill of Airy, Hill of Kittrick and Cairnsmuir, which last is considerably higher than any of the rest, and rises to 1737 feet above the level of the sea. The bounding line of the granite may be sketched in the following manner. It touches Loch Ken, two miles S.S.E. of New Galloway, continues to run along the brink of that lake for a mile and a half south, then passes a little more to the westward, a short way above the house of Bennan, and stretches away with no very great variety of direction to the hill of Sloughary, and across to the river Fleet. It then makes a gradual turn to the westward, round by Creetown, up the Burn of Palnure, past Craigdews, and by the high bridge of Dee, round the west side of Blackcraig, down by Nocknairland, about two miles south of New Galloway, and into the lake of Ken. This

Plate XVIII.

SKETCH
of the
Country
near
NEW GALLOWAY.



The Lines running nearly S.W. & N.E. mark the general direction of the stratified Rocks.

[illegible] for Dr. Thomson's [illegible] Published by [illegible] June [illegible]

space includes about ten miles by four. The river Dee traverses this district of granite from west to east, and affords a very fine opportunity of observing the meeting of it with the neighbouring rocks, at both the western and eastern side.

In the month of August last I spent some time in examining this granite mass. My object principally was to ascertain as accurately as possible the rocks with which it comes in contact, and the manner in which they and it meet one another. I was anxious to determine, 1st, Whether the greywacke or greywacke slate could anywhere be seen in contact with the granite, and if not, what the nature of the rock or rocks was which intervened between them, and whether any thing like primitive rocks intervened, such as gneiss, mica slate, or clay slate. 2dly, I was anxious to determine whether any appearance could be observed of granite resting on any other rock. 3dly, Whether there was any appearance of the stratified rocks in the neighbourhood being lapped round this mass of granite so as to give them what is called the mantle shape. 4thly, I had in view to observe the appearances of the granite veins.

In prosecution of these objects I left New Galloway on the 7th of August, 1813: and, 1. On the brink of Loch Ken, about four miles down, or towards the S. S. E., I found a rock very much resembling mica slate resting on the rock which lies immediately over the granite (afterwards to be spoken of), and running in a direction N. E. by N., dipping to the eastward, and at an angle of about 80°. The ends of the strata of this rock are to be seen on the high road close to the brink of the lake.

2. Half a mile farther south I found a rock which I shall for the present take the liberty to call fine-grained or compact gneiss, resting immediately on the granite, and running in the direction of E. N. E.; dip to the eastward; inclination about 70°.

3. One quarter of a mile farther to the south-west, on a third observation, found the compact gneiss running E. N. E.; dip and inclination as before.

4. Two hundred yards farther to the west, found the gneiss running N. by E.; dip and inclination as before.

5. A little way west of this I found ironshot gneiss running nearly north and south, still dipping away from the hill, that is, easterly, and inclined about 60° or 70°.

6. Half a mile to the south found the strata of the compact gneiss running N.N.E. nearly vertical. This observation was taken in the strata of a height nearly to the south of the Louran, and about half a mile from the north bank of the river Dee.

7. A quarter of a mile S.W. from this, compact gneiss running N.N.E. Here I observed a granite vein in the fine-grained gneiss, 18 inches wide, running nearly east and west. It is about 200 yards from the bank of the Loch of Strone, and half a mile east of the house of Clachrum. The strata of compact gneiss at this place seemed to be all much in the above direction, viz. N.N.E.; dipping to the south-eastward; elevation about 80°. These strata of compact or fine-grained gneiss seem to alternate with the granite

in beds of a great thickness. I measured one of the beds of the gneiss back from the granite toward the south-east, and found it to be not less than 25 yards thick. At this place there are a number of loose blocks of the rock which rests immediately on the granite of the Louran, and which some have termed an altered rock, or greywacke changed by the influence of the granite in a state of fusion, but which I think is better denominated a fine-grained or compact gneiss (the appellation given it by Professor Jameson). I measured one of the blocks of this rock, and found it to be *five* feet thick, and nearly double that in length. I cannot therefore understand a passage of Sir James Hall's ingenious paper in the last published volume of the Transactions of the Royal Society of Edinburgh, entitled, On the Conditions of Strata and their meeting with Granite, wherein he says, speaking of the Louran, "In the immediate vicinity of the granite, to the distance of a *foot or two, and not more*, the stratified matter has in many cases assumed a highly micacious character, so as to deserve the name of mica slate, or perhaps gneiss." This passage is quite inconsistent with the existence of loose blocks of that rock five feet thick, and still more so with strata of it 25 yards in thickness. Indeed, if I mistake not greatly, at the very place where Sir James, some years ago, with such laudable zeal for the interests of science, and such indefatigable perseverance, denuded a large portion of the rock on the east side of the Louran, at the place called Windy Shoulder, there are to be seen strata of this fine-grained or compact gneiss, (or, as Sir James chooses to term it, "stratified matter assuming a highly micacious character,") of a thickness far beyond what he states. I had here an opportunity of observing the very interesting granite veins of which Sir James has given so accurate an account, and which are thought by some to afford a strong confirmation of the truth of the Huttonian theory, but which others view in a different light, and consider as nothing more than contemporaneous veins. I found a variety of the fine-grained gneiss among the loose blocks I already mentioned near Strone Loch, with the mica much more distinct and abundant than any I had before seen in the district.

8. My next observation on the bearing and dip of the strata was on the brink of Strone Loch, very near the lower end of it, from whence the river Dee issues. Here I found compact gneiss strata running N.N.E.; inclination about 70, and dip towards the south-east; of a great thickness—upwards of 40 yards thick.

9. My next observation was about 200 yards below the foot of Strone Loch, where the compact gneiss crosses the river in vastly thick masses of nearly vertical strata, dipping a little however to the south-eastward, and running in a direction from N.N.E. to S.S.W. I was desirous, if possible, to discover some place where the rock was sufficiently exposed to enable me to see the junction of the gneiss with the greywacke: and I conceived that the bed of the river here was the most likely to afford me such an opportunity. I therefore traced it down about a mile, and at last I found a rock laid bare on the north side of the Dee, where the compact gneiss

and greywacke slate are to be seen very near to each other (within the distance of two or three feet); but I could not say that I was able to tell exactly where they met. The strata are in a direction N.E. by N., and are nearly vertical. There is no granite within a mile of this place.

It may be proper to mention that Strone Loch is a lake about half a mile long, and three quarters of that in breadth, through which the river Dee passes. The scenery around is of the wildest and most barren aspect, presenting nothing towards the north, the west, and the south, but granite mountains, some of them rising to the height of more than 1500 feet, and exhibiting a great deal of bare rock, both in rolled pieces, and large faces of the rock *in situ*, without any covering of soil. Where this does exist, heath is almost the only vegetable that in a general view may be said to strike the eye. The patches of green are few; and only two or three human dwellings, and these of the very humblest kind, are to be seen in the whole valley. The banks of the lake itself, however, are in general flat, and in many places soft, with a sort of alluvial or meadow ground, extending a little way (say 40 or 50 yards) from the water's edge, in some places, when this is at the lowest. In the lake grow many aquatic plants, as the common bulrush, *scirpus palustris*; pond-weed, *potamogeton natans*; white and yellow water lilies, as they are called, *nymphæa alba* and *nymphæa lutea*; horsetail, *equisetum*; and many others. It contains five species of fish: salmon, pike, eels, trout, and perch. I had no means of ascertaining its depth; but that cannot be great, as it readily freezes over by a night's frost or two. The river Dee, as before mentioned, falls into this lake on the west, and issues from it on the east, and very near the place where the granite and stratified rock are seen to join. Strone Loch lies south from the Louran.

Leaving this lake I proceeded up the banks of the river Dee for four miles till I came to the High Bridge, along which the great road passes from New Galloway to Newtonstewart. No rock but granite is to be seen along the bed of the river from the east end of Strone Loch to this place; but, as mentioned by Sir James Hall, there is here a junction of the granite and the stratified rock to the west. On this quarter a district of transition country of considerable extent (probably six or eight miles broad) divides the granite mass of the Dee from that of the Doon, or most westerly of the three granite districts of Galloway. The granite of the Dee district does not present much variety of either colour or size of grain; and the ingredients seem to be in general also united nearly in the same proportion. The colour of the quartz is generally greyish white, of the felspar greyish white or reddish, sometimes approaching flesh-red, and of the mica black or brownish black. The grain of the granite is not of a large size, nor yet very small. In general the crystals of quartz and felspar may not exceed a quarter of an inch diameter; but at times they occur very large. In some varieties schorl appears, and in others hornblende.

At the High Bridge of Dee, that is, 150 yards below the site of the old bridge, and about twice that distance above the new, a junction of the granite with the stratified rock appears, running across the channel of the river. "In one case," says Sir James Hall in the paper formerly alluded to, "which occurs in the bed of the river at the High Bridge of Dee, I saw the bounding surface of the granite dipping at an angle of 45° from the centre of the granite mass, and the strata lying upon it in what, in the Wernerian language, is called a *conformable position* to the granite, and corresponding exactly to what they have held out as the mode in which the granite always meets the strata."

11. I found Sir James's observation here to have been quite accurate. The stratified rock, corresponding as nearly as may be to the character of the fine-grained gneiss which rests immediately on the granite on the east side of this district, here lies upon it at the above-mentioned angle of 45°, and the direction of the strata is N.E. by E. The dip is north-westerly, just in the opposite direction it will be observed, that the strata dip on the eastern side of the granite. I here measured the thickness of the compact gneiss backward at least 40 feet from the granite. I mean to say that I measured the stratified rock to this distance, and in all that space could not perceive any difference in its appearance so as to induce me to consider it as a different rock from that which touches the granite.

12. Crossing the Dee, I now proceeded southward, by Tanerghie and Craigdews, towards the Burn of Palnure. About two and a half miles from the Dee, and one and a half mile north-west from the famous height called the Saddle-loup, which is just along Craigdews, I observed the greywacke slate exposed; and seeming to have a considerably different direction and inclination from any I had hitherto seen. On trial I found the direction E. by S.; inclination 20°; dip northerly; still away from the granite, which is here about half a mile from it. This observation was taken about half a mile south of the small lake called the Lily Loch, from the great quantity of the water lilies, as they are called (*nymphæa alba*), it contains.

13. Half a mile farther to the south I found greywacke slate in the burn at the house of Tanerghie, running E. by N.; inclination 35°; dip westerly. Proceeding a short way farther south from the house of Tanerghie, we came to a small lake. The hill rises very suddenly on each side of this lake, both to the east and west. The formation is all the greywacke and greywacke slate. Along the banks of this dreary looking little lake, and past Tanerghie, the old road used to go from Newtonstewart to New Galloway. The valley is lonely, gloomy, and dismal, from the narrowness of the glen, and the barren aspect of the mountains, so that travellers in former times, during their nightly tramps through it, were frequently annoyed by the sight and sound of ghosts or hobgoblins. A little way to the south of the lake the Burn of Palnure precipitates itself

from the hill on the west through a deep ravine, and over a perpendicular face of rock 18 feet high, forming a very beautiful and interesting cascade; and as it was within sight of the old road from Newtonstewart to New Galloway, it caught the attention of travellers, and usually got the name of the grey-mare's-tail. The direction of the strata here is E. by N.; dip 45° westerly: and there is, in the perpendicular face over which the water falls, something like an immense globular concretion of very hard greywacke, having a soft slaty rock both above and below it.

Descending along this burn, which turns towards the south-east, I soon came to two more waterfalls on it, equal in height and beauty to the grey-mare's-tail. These are near to one another a little way to the N. W. of Craigdews, or the Craig of Firs, as it properly signifies in the Gaelic language; for *ghews* is the name in that language for fir. Craigdews is a beautiful dell at the bottom of the steep and narrow ridge called the Saddle-loup, over which in former times the high road passed from Ireland to Newgalloway. There are fir trees of a great age growing at the bottom of this hill, and hence probably the name of Craigdews, as there are no other trees of the same sort, indeed scarcely any other wood at all near this place. The rock is all greywacke or slate. The burn of Pulmire after passing Craigdews flows along a valley of little descent, on the west side of Cairnsmuir, and comes in contact with the granite of that mountain about a mile below Craigdews.

14. Here I took another observation, and found the stratified rock running N. E. dipping north-westerly at an angle of 45°. The grey wacke here appeared to me to come in contact with the granite, and in some degree to alternate with it. If any thing like compact gneiss intervenes here betwixt the greywacke and the granite, it is very thin. I observed distinct greywacke within less than 10 feet of the granite. I traced the burn for a mile and a half farther down, and could see in other places junctions of the granite with the stratified rock, the strata all in the usual direction, and dipping from the granite; the inclination of the strata about 45 or 50. I regret that I had it not in my power to have proceeded farther on, as the track of this burn, which is of a considerable line, (about the magnitude of the water of Leith,) seems to keep in a great many places the line of the junction of the granite and shistose rock, and thereby present favourable opportunities of observation. The scene is wild and sublime: for on the east you have the granite mountain of Cairnsmuir rapidly rising to the height of 1737 feet, and to the west the transition country, almost equally bold and precipitous. There are in the bed of the rivulet many large rolled, granite, and other blocks; and it was, I believe, in some of the former that Professor Jameson three years ago discovered zircon. On ascending the granite mountain for a considerable way, I found rolled pieces of greywacke nearly half a mile up, and at least 200 feet above the level of the highest point where the two sorts of country join. I may here observe also, that I

found to the eastward of the Louran on the opposite side of the lake of Ken, a great way above its level and at the distance of 3 miles from the granite, several rolled blocks of that rock. It is known that no granite rock occurs to the east of Loch Ken in this district.

On turning northward again along the west side of the granite mountain of Cairnsmuir I came to Kittrick, a place of seemingly little note certainly, but now become interesting by having been the birth place of the late Dr. Murray, Professor of Oriental languages in this University: a man well known to have possessed unrivalled talents for the acquisition of languages and for philological research, having made himself master, it is understood, of no fewer than 15 or 20 languages, so as to be able to translate from them with certainty, at a very early age, and who was to the inexpressible regret of all who knew him, and of the literary world in general, snatched away by death at the age of 36. Kittrick is the name of a sheep farm belonging to James M'Kie, Esq. of Bargally, situated in the parish of Monigaff, and includes in it a portion of the Cairnsmuir range to the north, called the hill of Kittrick. This hill is little more than an immense mass of bare granite rock, with a little covering of heath here and there; rising suddenly to the height of 1000 or 1200 feet. At the bottom of this hill on the north-west, in as wild and desolate a looking spot as perhaps ever human dwelling was seen to occupy, stood the cottage or rather hut in which Dr. Murray was born. The cottage was built of granite, and no part of it now remains except the walls. They are situated little more than half a mile from Craigdews, and within sight of the new line of road which has been made from Newton Stewart to Newgalloway; and from which latter place Kittrick is distant 13 miles. The ruins of the cottage are within a quarter of a mile of the road. Dr. Murray's parents lived here for 14 years, and his mother, who is still alive, tells me that he was born eight months after they came to the place. The occupation of his father was that of a shepherd. On this wild and sequestered spot, secluded one would think almost totally from the intercourse of mankind, did our late great philosophic linguist spend the first 13 years and four months of his life.

About 30 yards from the ruins of the cottage is a large rolled block of granite, 15 or 20 feet high, with a perpendicular flat face looking towards the high road. It struck me that it would be a proper piece of respect to the memory of Dr. Murray, and a monument more lasting perhaps than any other that could be erected, (except his own writings,) to have engraved deeply on this granite block, in large letters, some such inscription as the following:—"Alexander Murray, D. D. Professor of Oriental languages in the University of Edinburgh, and the greatest linguist of his age, was born in a cottage not many yards from this stone, in the year 1776, and died at Edinburgh on the 23d of April, 1813."

Returning towards Newgalloway I took two or three more bearings of the strata on the north-west side of the granite district, which terminates on that quarter in a high round-topped mountain called Blackcraig. I found them all to be directed about E. N. E. dip 35° westerly. It appears then that the stratified rock all around the northern half of this granite mass has the direction of its strata very nearly the same. It varies only six points, viz. from E. N. E. to N. by E. In one case, as I mentioned, I found the direction three points more easterly than any other I had seen, but this was distant from the granite. It appears, also, that on the east side of the granite mass the strata dip easterly, and on the west side westerly; or in both cases away from the granite. But on the north-eastern side of this mass the ends of the strata run directly towards it.

There is in this district of country, that namely called Glenkens, which is the valley of the Ken from the borders of Ayreshire towards Dalmellington, down part of the Louran, a remarkable change in the dip of the strata. All about Newgalloway, and, as far as I had the opportunity of seeing, towards Newton Stewart on the western side of the granite mass, the dip is northwesterly; but after we pass Carsphavin, which is about 13 miles N. W. by E. from Newgalloway, the dip of the transition strata changes, and all along from thence to the junction of these with the sandstone country of Ayreshire, 10 miles farther on, and within two miles and a half of Dalmellington, dips south-eastward. In Glenmuck three miles and a quarter south east of Dalmellington, I observed in the greywacke a small bed of transition limestone with much magnesia, four feet thick, dipping to the south-east at an angle of 70°.

In various parts of this district of transition country, called Glenkens, I observed large beds of felspar porphyry, or what I was at one time disposed to consider as transition greenstone. The felspar is of various colours; but most commonly of a reddish cast, something perhaps between brick red and flesh red, but it is sometimes greyish white and greenish grey: it contains hornblende. A few of these beds are situated as follows. In the channel of the burn of Halfmant opposite the church of Carsphairn, is a bed of this rock traceable for 300 yards, and about 20 feet thick, nearly vertical, reddish at the south-westerly end, but on the end next the north-east greenish grey. At Darnaw, 12 miles from Newgalloway towards the N. W., is another bed of this rock 20 feet thick, containing very little horneblende, and of a greyish white colour. It is exposed to view for 100 yards. The common people took it, as well as the above mentioned bed at Halfmant, for limestone; and specimens of it, which I found had been procured by blowing it with gunpowder, were actually sent to Lord Glenbee at Barskimming under this idea, viz. of its being limestone. Another bed of the porphyry occurs in the channel of the river Deugh, which falls into the Ken four miles below Carsphairn. It appears at the bridge where the high road passes, is very red, and is called by the common people *red granite*, as are indeed generally the other beds of this rock throughout the country.

A fourth bed 50 feet thick, nearly vertical and of a red colour, is to be seen running across the river Ken at the head of the Isle of Cleugh, about two miles farther down; and there is another still farther down, about half a mile above the house of Todston, crossing the river also, of about the same thickness, and inclined at an angle of 60.

About 300 yards up the river from the first of these two beds of porphyry, I observed a remarkable vein of quartz in the greywacke three feet thick and traceable for 40 yards, cutting the strata very nearly at right angles. In this vein there appeared to be also felspar, and quartz mixed with greywacke.

I observed several other smaller beds of the porphyritic rock in this district; but these it is unnecessary to particularize. I shall mention just one more, which occurs in the Fell, as it is called, of Mochrum, about three miles to the east of the Louran. This Fell is a conical hill, in the transition country, rising considerably higher I should think than the Louran itself; (for I do not know the measurement of it.) I estimate its height at 1000 feet. It is far from any other hill nearly so high as itself, and consequently commands the finest view that I think is any where to be had of the whole valley of the Ken. From Mochrum Fell can be seen, when the atmosphere is tolerably clear, almost the whole of the lower part of Kirkcudbrightshire, the sea, with the island of Little Ross at Kirkcudbright itself; Sanbee's Head in Cumberland, all along the Solway firth by Workington, and as far to the east as Dumfries. On the south side of this Fell is a large bed of porphyry, traceable 100 yards and 40 feet thick, the usual direction of the strata and dipping to the south-west, inclination 60°.

On the 2d of September I went to the Louran, for the purpose of collecting a suite of specimens of the rocks of that hill for a friend in Edinburgh, and, the day being fine, had a most interesting view of the surrounding scenery. The prospect from this hill will I believe be allowed by all who have seen it, to be very fine. Seven lakes are in view, viz. Loch Ken,* Strone Loch, Loch Scarrow, Black Loch of Bennan, Woodhall Loch, Bleach Mill Loch, and the Loch of Barnboard; together with the whole sweep of the fine valley of Ken, from the borders of Ayreshire to Castle-douglas; a part of the Solway firth, and the Cumberland hills beyond it. On the west and south are the towering granite mountains of Cairnsmuir, Achar, and Blackcraig, with the River Dee precipitating itself through the rugged valley betwixt the two latter.

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* The kinds of fish that occur in this lake are the same with those formerly mentioned as found in the Loch of Strone, viz. salmon, trout, eels, pike, and perch. Some of the two latter sorts arrive at a very great size. I have very often killed in Loch Ken perch weighing four pounds, and at one time a pike of seven; but this is nothing in comparison of one that was caught about forty years ago in this lake, by John Murray, Gamekeeper to the Hon. John Gordon of Kenmore. It weighed 61 pounds, and the head of it is still preserved in Mr Gordon's library at Kenmore Castle.

The top of the Louran is formed of a rock of a slaty structure resting immediately on the granite, the strata in the usual direction, and dipping towards the south east at an angle of 70°. It contains much magnetic pyrites, and constitutes a sort of mountain cap of about a quarter of a mile long from north to south. On one part of this cap I found the magnetic power so strong as to make the north pole of the needle stand as nearly as I could estimate, S. W. by S. By shifting 10 yards to the north or south of this point, the needle regained very nearly its natural position.

I shall add to these imperfect notes a fact respecting the preserving power of peat moss, communicated to me by James Carson, Esq. of Barscoke near Newgalloway. Twenty-one years ago, the former proprietor of his estate, the late Mr. Fraser of Gantuleg, in making a road through a moss, had laid in a quantity of branches of trees, birch, hazel, ash, willow, and hawthorn, and covered them completely over with the moss. Mr. Carson lately had occasion to take them up, and found, he says, the branches not more decayed than they would have been by lying one month above ground.

A friend of mine lately, whilst his people were digging peats in a moss not far from the same place, found a large quantity of hazel nuts (about two English quarts) placed within a cubical cavity, formed by six broad stones three feet below the surface of the solid mass. I have been able to recover two only of these nuts as a specimen.

Edinburgh, 14th Jan. 1814.

Article IV.

On Iodine, Chlorine, Fluorine, &c. By M. Van Mons of Brussels.*

A new acid has just been discovered in France. It exists in the state of a salt in kelp. It may be obtained by treating the ley of kelp, (deprived of its alkali,) previously heated a little with sulphuric acid. A heavy substance precipitates of a black colour, and possessed of more or less brilliancy. It is an oxygenated acid.

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* I received the paper of which this is a translation some weeks ago from the author. As it is without date I have no means of knowing when it was written. The author is unacquainted, I presume, with what has been done in Paris and London respecting iodine. His views are particular, and I conceive not altogether accurate. His method of procuring iodine does not correspond with ours. His name *varine* (from *varec, kelp*) is peculiar to himself.—T.

The sulphuric acid supplies it with oxygen, and is itself converted into sulphurous acid. It is only in this form that the substance in question can be separated from the oxides, and not in the state of a complete acid. We may at once distinguish the source of this new body and preserve its analogy with *chlorine* and *fluorine*, by giving it the name of *varine*, *varic acid*, *varate*, *gas varinique*, *&c.*

Varine sublimes at a heat below 212°. It then crystallizes and assumes the colour and metallic aspect of native sulphuret of lead. Its vapour has a fine violet colour, and a smell analogous to that of chlorine.

With hydrogen varine forms a gaseous, colourless acid, having a smell somewhat similar to that of muriatic acid. With the metals it forms dry *varates*, and with their oxides *oxygenated varates*, which give out oxygen gas at a red heat, and which detonate when mixed with phosphorus, sulphur, &c., and struck with a hammer. The *hyperoxygenated varate of ammonia* may be obtained directly by mixing the liquid alkali with varine. This salt is almost insoluble in water, it has the appearance of a black powder. When dry it detonates on the slightest friction.

Varine combines with phosphorus and forms a compound similar to sealing wax, which is converted by water into varic acid and phosphorous acid.

Water does not separate varic acid from oxygen; but oxygen separates it from water. This shows us that this acid is more of a combustible nature than a supporter of combustion. There appears to exist a compound composed of varine and varic acid gas. The oxymuriatic acid of Berthollet is doubtless a similar combination.

Varine combines with chlorine and forms a solid crystallized body of a yellow colour, which attracts humidity from the atmosphere. Varic acid in the state of gas exchanges its water for the oxygen of chlorine, and the varine manifests its presence by its beautiful colour.

Alcohol and ether dissolve varine, and no sensible change is produced either in these liquids or in the varine.

Nobody has hitherto separated the oxygenated varine from the hyperoxy-varates. This is natural, as the varic acid is not separable from bases by other acids. Most acids expel chlorine from its salts; but oxygenated chlorine separates sulphuric acid from ammonia when it forms the *detonating oil*, which is beyond doubt a hyperoxymuriate of ammonia, and the very same substance which I obtained and made known 20 years ago.

When this detonating oil is directly exposed to the light of the sun under water, oxygen gas in abundance is given out, and there remains common muriate of ammonia. This fact, joined to the circumstance that muriatic acid when in sufficient quantity becomes oxygenated, and when in smaller quantity becomes hyperoxygenated, puts the nature of this body beyond all doubt. The hyperoxygenated varate of ammonia will no doubt exhibit the same phenomena. It is impossible that the first of these bodies should be a compound of azote and chlorine, and the second of azote and varine, for such combinations are far from being spontaneously decomposable, and with the disengagement of heat would be retained by affinities as strong as the oxides of phosphorus, sulphur, and carbon; as they would consist of a combination between an acidifiable combustible and an oxygenated acid, which could only exist in consequence of a very considerable displacement of caloric. Besides, water would resolve such compounds into muriatic or varic acid and azote, more or less oxidated. The oil would be dry *aqua regia.* You must perceive that the direct formation of hyperoxymuriate or hyperoxyvarate of ammonia, destroys the whole theory which regards *chlorine* and *varine* as simple bodies, unless we admit ammonia to be a metallic oxide; for it is only by the decomposition of that oxide, that oxygen could be supplied for the superoxygenation.

I have found a combination between potash and oxygen, which water is not able to destroy without the assistance of a strong heat. Hence the oxygen is much more condensed in this compound than in the ordinary peroxide of potassium. It is obtained by heating in a retort a mixture of crystallized caustic potash and red oxide of mercury till the metal is reduced. There remains a white crystallized salt, which is a hyperoxide of potassium.

The subhydrates of potash and lime condense atmospheric air without decomposing it; and when heated after this absorption give out pure azote, the oxygen remaining combined with the bases constituting them oxygenated hydrates.

I have combined dry fluoric acid (or *fluorine* without oxygen, or *fluore* without hydrogen) with the metals. They constitute powders, of a more or less dark colour, which absorb oxygen when heated, and are converted into dry fluates by the oxidation of the metals. Water separates the dry acid and leaves the metal unaltered, when it is not oxidable by water. When it is oxidable fluates are formed. These are true salifiable combustibles, as the hydrogenated acids are acidifiable combustibles; and the same bodies oxygenated supporters of combustion of that quality. In the first the oxygen converts the metals into oxides, in the second the same principle converts the hydrogen into water, and in the third the hydrogen converts the oxygen into the same liquid.

The oxides, as water, separate the metals from these compounds, unless they be capable of oxidating them.

We obtain the metallo-fluors by heating a mixture of fluor spar, sulphuric acid, and an energetic metal reduced into leaves or filings. They may be obtained likewise by passing a current of hydrogen gas on the fluates of the old metals, placed out of the contact of common air. The metals are reduced, and a portion of the acid unites with them in preference to the hydrogen. They are obtained likewise by the action of the alkaline metals on silicated fluoric acid gas. The earth is separated, and the dry acid unites with the metals, forming fluors of potassium and sodium. Under the action of the galvanic battery, fluoric acid and all its compounds, when moistened with water, generate the same products. The dry acid accompanies oxygen to the positive pole, and preferring the metal of the wire to that principle, unites with it, and forms fluor of platinum, &c., unless the wire be oxidable, in which case it forms a dry fluate.

From these facts it appears that dry fluoric acid is neither *hydrogenable* nor *oxygenable*, but only *metallable*. It is analogous to the oxygenable acids, *chlorine* and *varine*, in forming dry salts, except that of ammonia, which the muriatic and varic acids do not form equally dry. Dry chlorate, varate, and fluate of ammonia, would be resolved by heat into azote, and into chlorine, varine, and fluorine, or into the acidifiable combustibles of the respective acids of these salts.

Potassium is not able to reduce the acids of the dry salts into the acidifiable combustibles, because the hydrogen which water exchanges for the metal is wanting in these compounds.

The dry acids contain oxygen less hydrogenated than in water, but more so in the oxygenable acids, than in those that are hydrogenable. They are superoxidated or burnt by oxygen, as they oxidate or burn hydrogen and metals; and they superoxidate water and the metallic oxides as the acidifiable combustibles are acidified. We acidify the suboxide of hydrogen or the oxide of this principle in these bodies, without combining them with the reduced bodies forming oxidated acids. With all other bodies they form salts.*

Article V.

Upon the dreadful Effects of the Explosion of carburetted Hydrogen in Coal Mines. From an anonymous Correspondent at Newcastle.

Amongst the many commentators upon this subject, there has not fallen under my inspection the production of any (with the exception of a small pamphlet by D. Nield) who has either entered into the cause of its formation, or who has proposed, or even hinted at a plan for removing the source of its production. Willing to allow to an ingenious correspondent of the Author of the *Annals of Philosophy*, all the merit that is his due for an invention to prevent accidents of this nature; which a correspondent Φίλελεεινός, of August 1813, expresses his earnest desire may be put in practice, if it were even allowing Dr. C.'s lanthorn to be an effectual remedy where the gas is already generated, it must be acknowledged that prevention is better than cure, and if we could remove the cause, the casualties that such a piece of mechanism, the contrivance of Dr. C.'s, must always be liable to in the hands of ignorant workmen would of course be obviated. That some method for the attainment of this object may be devised and adopted every one must allow is desirable, and it is only by observation and a liberal communication of sentiment that such means are to be found out; it is with this view that these hints are communicated, in hopes that some person, with more opportunities of observation and better qualified, may pursue the subject to its attainment. In the annals of calamity from the explosion of carbureted hydrogen in coal mines, there is scarcely a parallel to the sum of misery that has been produced by two explosions of Felling Colliery, within the space of 19 months; for a minute and interesting account of the first, see an excellently written pamphlet by John Hodgson, printed at Newcastle. This explosion happened 25th May, 1812, by which ninety-two human beings were deprived of existence! The last in January 1814, by which near 30 lost their lives, and several severely injured. This pit is situated on the south side of the Tyne, and about two miles below Newcastle bridge; the coal that is obtained from this and the adjoining pits evidently contains a large proportion of pyrites, or iron combined with sulphur; so much so, that at an adjoining pit, I have seen a considerable pile of large pieces which have been selected as bad coal. Not more than 500 yards from the pit itself are the remains of an old pit heap, (or small coal which it is the custom to separate from the coal sent to London,) which within these five years yet showed signs of combustion, though probably near half a century has elapsed since its first perhaps self-ignition, (for this is sometimes the case.) The ashes of this confined heap of coal are a bright brick red colour, no doubt from a large quantity of iron reduced to a red oxide; and I have seen in fissures of the heap from which smoke issued, crystals of sulphur. The effect produced by moistening sulphur and iron in the state of a mechanical mixture is too well known to need comment; but it is perhaps not so generally known, that a proportion of the scum of coal in every pit is allowed by the owners to be worked as small by the pit men, and for this they are paid by calculation at the same rate as the large coal; it is considered as refuse, and consequently left in the old workings of the pit, with a most extended surface presented to the action of the water, which is always present in a more or less extensive degree: the consequence is, that impure hydrogen is evolved in such immense volumes, that in defiance of the boards which they carefully but ignorantly put up to keep this enemy undisturbed, it in the end acquires such an overwhelming power, as to break through its confinement, and, in the manner of other fluids soon increasing the first aperture, rushes out, and, like the springing of a mine, without a moment's warning produces effects to which the wretched remnants of the inhabitants of the village of Felling can but too well bear testimony. That it is impossible to prevent such occurrences, even by the most vigilant attention and the adoption of the very best plans for ventilation, this pit would appear to be a sufficient proof, as in these respects it ranks in character with the best. Ought not then the removal of this small coal to become an object of immediate attention? It might indeed be worthy of observation, whether the bottom of the scum (the part that is left in the pit) contains a greater or less proportion of this combustion, so harmless in its simple state, but so capable of being converted in the laboratory oven of nature into a state so pregnant with destruction. The coal owners say they cannot afford it. If this be the case, may not means be adopted without oppression, to compel the men to bear a part or all the expense attending it? The depth they are allowed to work into small coal is about nine inches, this in an average seam in the neighbourhood of Newcastle, say five feet, would require nearly one hour in seven to bring the small coal out of the pit; and might not this be sacrificed? would it not be attended in many respects with mutual advantage? it would do away that responsibility attached to the office of an overseer, whose duty it is to compute, or rather to guess by measurement, the number of baskets of small coal that are thus laid to one side, (the pitmen being paid so much a corve or basket,) it would prevent the owners and the men alike from error or want of integrity; it would be a stimulus to the men to work the less small coal if they had to pay for its being raised out of the pit. Thus might compulsion be applied; for that it will be necessary to resort to compulsatory measures I have little doubt, from the known obstinacy attending ignorance in similar cases. Such measures, if carried into effect, might be confined to pits that have been known to explode within a given time, or new shafts within a given distance of such. Perhaps those coal districts where similar accidents (or rather I would call them melancholy occurrences) have taken place, exclusive of the neighbourhood of Newcastle, might better afford to bring up this small coal, from their strata being deeper. Wherever copperas is manufactured there will be a danger of these explosions, and this might be one grand guide for the limitations of an act; but upon these ulterior subjects I would but slightly remark, hoping that these undigested hints may lead some one to a deeper investigation, that will ere long attain "a consummation so devoutly to be wished."

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* I have translated this paper as literally as possible from the original French; because there are several parts of it which I do not fully understand. M. Van Mons uses a nomenclature of his own, with the import of which I am not sufficiently acquainted to turn his language into intelligible English.—T.

Newcastle, Feb. 1814. C.

APPENDIX BY THE EDITOR.

Respecting the production of *carbureted hydrogen* in coal mines, I am afraid that little satisfactory can be offered in the present state of chemical science. Whenever vegetable charcoal is left in contact with water at a certain temperature, the water is decomposed and two new gases evolved, namely *carbureted hydrogen* and *carbonic acid.* This happens during summer in all marshes and ditches, as is well known to every chemist. It is by this process of nature alone that pure carbureted hydrogen can be obtained for chemical examination. I conceive a similar process to be going on in coal pits, where the temperature is always high, and where abundance of coal is always lying in contact with water. I do not see how the presence of pyrites in coal should occasion or increase the evolution of carbured hydrogen, which there is every reason to consider as the only *fire damp* that ever makes its appearance in coal mines.

I wish the proprietors of coal mines would turn their attention to some circumstances, which if duly attended to would enable them completely to put an end to the disasterous effects of explosions of fire damp in their mines. These circumstances are the following: 1. Explosions of fire damp are entirely confined to deep coal mines, and never happen in those at no great distance from the surface of the ground. Thus nobody ever heard of such explosions in the neighbourhood of Edinburgh or Glasgow; but about Borrowstoness, where the mines are deep, they occur as well as in England. 2. The specific gravity of carbureted hydrogen gas is only 0·555, or a very little more than one half of the specific gravity of common air. 3. If you let go ever so much carbureted hydrogen gas in a room with an aperture at the roof, and examine the air of the room half an hour after, no traces of the carbureted hydrogen will be detected. 4. Carbureted hydrogen will not explode unless it amount to $\frac{1}{12}$th of the bulk of the common air with which it is mixed.—The unavoidable conclusion from these facts is, that if the fire damp accumulate in coal mines so as to explode, it is only because circumstances prevent it from making its escape with sufficient rapidity. Hence it follows, that the defect lies in the mode employed at present to ventilate coal mines; and that if the mines were ventilated according to the well known principles of hydraulics, no explosions ever would take place.

To prevent the evolution of fire damp I conceive to be impossible; to attempt to destroy it when formed, as has been sometimes proposed, is quite absurd; but allow it to make its escape from the mine without obstruction, and it will occasion no inconvenience whatever.

Article VI.

Account of a singular Case of a Man who vomited a urinous tasted Liquid. By W. Reid Clanny, M.D. M. R. I. A. Hon. Member of the Royal Physical Society of Edinburgh, and Physician to the Sunderland Dispensary.

As I understand that at present a controversy is carried on in one or two of the London Medical Journals, upon the singular subject of a female who was supposed to vomit her urine, I beg you will give a place, in your *Annals of Phisosophy*, to the case of Ralph Cooper, who lately became a patient of mine in the Sunderland Dispensary; which may perhaps throw some light upon a subject, which has long before this time attracted the attention of medical men; and from the very interesting information which your Journal has afforded upon the chemical analysis of animal fluids, particularly of those of the human body, I hope the following case will not be unacceptable to your numerous learned readers. From my not having as yet read the controversy which I hint at above, it will readily be granted, that the communication which I am now about to make will be found as disinterested as it is authentic.

Ralph Cooper, æt. 24,
Ph. Pulmonalis et anasarca. Admitted May 27, 1813.

This young man is well formed but delicate. His feet and legs are much swelled and hard; the urine is in very small quantity and high coloured; the pulse is frequent and quick in the beat. He is tormented by a severe cough and purulent expectoration; and not long since the sputum was mixed with blood, and frequently the hæmoptysis has been severe. By the use of digitalis purpurea in substance; supertart. potassæ; diluted sulphuric acid, and occasional opiates and laxatives, he has been greatly relieved, though the relief is not permanent; for the least exposure to cold or damp produces a return of all his bad symptoms.

It is not needful to record here the daily and weekly practice; as it would not only greatly enlarge the communication, but also distract the attention from that particular phenomenon to which I wish to draw the readers. I was called to visit him upon the 12th of December, when he informed me that from the medicines which I had ordered him he was so much relieved, that he considered himself as restored to health, and that a few days before he had been working very hard as a caster of coals upon the river Wear, which had produced a return of his former complaints to a much greater degree than hitherto. His legs and thighs are considerably swelled, the abdomen is much distended, and shows all the symptoms of ascites abdominalis. The urine does not exceed a pint in quantity, which is very high coloured, and upon cooling a lateritious sediment is deposited. The bowels are pretty regular, and the pulse fluctuates between 115 and 120. With these symptoms, the expectoration is purulent and copious. The appetite is very bad and frequently depraved. In a word, there is not one favourable symptom, and his dissolution may be expected at no very distant period. It is his wish as well as mine to obviate particular symptoms, so as to render him comfortable.

20th December. Since my last visit he has been attacked several times by severe and incessant vomiting: he observed that the quantity of liquid which he vomited greatly exceeded the quantity of liquid which he had swallowed for several days before; while at the same time the urine which was passed by the urethra was stationary as to quantity, and tolerably healthy in appearance. He describes the liquid which he vomited as exceedingly offensive, and to be of a salt and urinous taste. He was astonished to find that the feet, legs, thighs, and abdomen gradually diminished in size during the time of the vomiting, which must have been effected solely by the vomiting, in seven days; for as was stated above, he was then using no active remedies, which appeared to be quite unnecessary in his very deplorable state. He also reports that the quantity of water which came off his stomach must have been to the extent of several gallons. The bowels have been and still continue regular.

Under these circumstances I became an anxious spectator, except that, when needful, antispasmodics were directed to alleviate the very uncomfortable state of the stomach.

January 5, 1814. This day I found the patient relieved from all dropsical symptoms, but in a state of great debility; the pulse 82, weak, soft, and regular. He is able to sit up in the afternoons. The urine is in sufficient quantity, and approaches a natural state. He is not thirsty as formerly, though he finds it needful to rinse the mouth at different times in the day with tamarind-water. His appetite is very bad, and he spirts piss to a considerable extent, without any difficulty. He remarks that after the distressing vomiting ceased, he was tormented by a severe itching over the whole body, particularly upon the hands, which now appear as if they were affected by an herpetic eruption. He has no uneasiness of the thorax, but from indigestion he has pain in the region of the stomach. The vomiting is at times troublesome, though he does not observe that there is more liquid vomited than he had previously swallowed. A distressing diarrhœa has commenced, which is occasionally relieved by opium pills.

It is much to be regretted, that I had it not in my power to examine the liquid which he vomited in such large quantities, as a chemical analysis would have demonstrated its affinity to urine; but from not knowing of the very singular and unlooked for turn which took place, till after the vomiting had ceased, such a desirable circumstance was entirely frustrated.

The only cases of a similar nature which I have read, are to be found in Dr. Percival's Essays, Medical, Philosophical, and Experimental, at page 375, vol. 1st, 4th edition; where the case of a female is narrated in his easy, natural, and unaffected style. In the Medical Facts and Observations, vol. 6, page 212, the case of a female is well delineated; besides these, the case of a nun and of a monk, are to be found in the Histoire de l'Academie Royale des Sciences, Années 1715 and 1722. But in none of these cases is any mention made of the salt and urinous taste of the liquid which was vomited, as in the case of Ralph Cooper; and here the theory of "the regurgitation of fluids in the absorbent vessels," which was broached by that superior youth, Mr. Charles Darwin, naturally presents itself to the mind: of his theory, and the experiment made upon his friend, an ample account is to be found in that excellent practical work the Medical Commentaries, vol. 7, page 193. Having swelled this paper far beyond what I intended, I shall for the present lay down my pen.

ARTICLE VII.

On the Distribution of the Inhabitants of Russia.

By C. T. Hermann.*

PART I.

Distribution according to the Nations.

THE total population of a country makes us acquainted with its physical force; the distribution of that population gives us its moral force.

Those people who are sprung from the same origin usually speak the same language, and have the same manners and customs. They understand one another, resemble one another, and consider themselves as members of the same family. The more savage or barbarous a people is, the more does this difference influence its conduct towards strangers. It is very difficult for Government to efface these characteristic distinctions, in order to establish the necessary union in a political body composed of different nations. The progress of knowledge certainly diminishes the effect of these national distinctions. Hence it happens that the higher ranks in all nations have a considerable resemblance to each other: but knowledge is not easily diffused among the lower orders of society. The most enlightened governments have endeavoured to destroy these distinctions. Russia has at all times followed this great principle. The new divisions of France had the same object. England has at last admitted the Scotch and the Irish into her Parliament.

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* Translated from the Memoirs of the Imperial Academy of Sciences of St. Petersburgh, vol. iii. Published in 1810.

Religion for a long time had a striking effect upon politics. From the end of the 15th century to that of the 17th the character of the politics of cabinets was religious. The 18th century bears the character of the mercantile system: and that of the 19th is revolutionary. Various governments have adopted the principles of toleration: but in some states it is political, without being religious; in others religious but not political. It is only in France, in Prussia, and in Russia, that it bears the double character of religion and politics.

The distribution of population according to the nations is one of the most interesting statistical inquiries. The farmer is attached to his fields, because upon them he has lavished his labours and the fruit of his savings. These fields are the only sources of riches, and consequently the possessors of them become by degrees the absolute masters of those that have none. Manufactures and commerce open a new source of riches independent of the territorial property. A third class of citizens interposes itself between the labourer of the fields and the proprietors of estates. They are justly called the third estate. They belong to the whole world. Knowledge and the arts friendly to liberty, comfort, and tranquillity spread with the greatest facility in this class. The want of the third estate stops the progress of knowledge among a people of slaves; and the German nations, notwithstanding their feudal system, were only more fortunate in possessing this third estate some ages before other nations. The nobility and the clergy form a political body between the sovereign and the nation. Their number, their property, their privileges, require the greatest attention in order to be able to judge of the moral force of monarchies. The great armies kept up by all nations have established a military system in the midst of peace. This system, brought to perfection since the time of Louis XIV. and Frederick II., has destroyed the finances, and overturned several states.

Formerly there were various states in Europe in which the sovereign was limited by the privileges of the people. Those provinces which had preserved *particular rights* sometimes rendered the operations of government more slow and more difficult.

The origin of nations, then, religion, the different orders of society, and the particular rights of certain provinces, are the principal points of view under which we are about to contemplate the total population of Russia.

Ethnography makes researches into the origin of peoples, and the smallest tribe is classed apart, provided it exhibits national differences.

The writer on political statistics attends to these differences only when they have a marked effect upon the happiness of the state.

Under the first point of view Russia contains nearly a hundred different nations; under the second, European Russia includes only three nations, the Sclavonians, Finns, and Tartars. We might indeed include the inhabitants of Caucasus; but they are not numerous. Siberia, besides the Finns and Tartars, includes likewise the Samojedes, and the people of the Mongole and American race. But this population is only in its infancy.

1. The centre of European Russia is inhabited by the Russians. On the west and south-west are found the Poles. We shall not uselessly multiply the subdivisions of the Sclavonian race by stating particularly the inhabitants of Great and Little Russia, the Cossacks, Serbes, Wlachians, Albanois, Arnautes, Bulgarians, &c. which occur as foreigners or colonists in the governments of the south. How many subdivisions of this kind might be made in France and England.

2. All the north of Russia, from Finland, by Archangel, Olonetz, Petersburgh, Novgorod, Wologda, Waetka, and Perm, is inhabited by Finlanders. Their numerous tribes are spread over the west and the east. In the west, by Esthlande and Livonia, as far as Courland; in the east, by Kasan, Nigegorod, Simbirsk, Resan, Tambow, Orenburgh, Saratow. They have passed the Oural, and are spread in the government of Tobolsk.

3. The Tartars occupy the south of Russia and of Siberia; the Tartars of Kasan, of Astrakan, of the Crimea, of Caucasus; the Tartars of Tobolsk, of Tschoulym, Buchares, Teleutes, Abinzes on the Ob, the Tschoulym and the Tom; foreign Tartars of Chiwa, of Persia, of Turquestan; Nogaens in the Crimea and on the Couban, Baschkines, Metscherjaeques, and several other tribes mixed with the Tartars and the Finns.

The inhabitants of Caucasus are classed apart, but chiefly for the purposes of ethnography.

1. The Samojedes are the first nation of Northern Siberia. Their tribes extend from the Frozen Ocean along the Jenisei, as far as Baikal, and stretch from the Ob very far into the eastern parts of Siberia.

2. Their neighbours are the American tribes, the Tsuktsches, the Kamtschadales, and the inhabitants of the Aleoutes and Couriles Archipelagos.

3. In the south of Siberia occur different tribes of the people called Mongoles.

The distribution of the population of Russia cannot be stated with the same accuracy as in Austria, where the different nations have different privileges. The Russian government having given to all its subjects the same privileges, and imposed on them the same duties, never requires from the governors information respecting the national differences. Of consequence the statements of the population in 1796, 1802, and 1804, and several other particular reports which I have consulted, give us no information on the subject. Their principles of division are financial and military. The statements of the population of Siberia have more of this kind of facts, because they are necessary there in a financial point of view. I ought to repeat here that all my calculations are founded on the statements drawn up by order of government, which are always th most probable. I know well their imperfections; but I am awa also of the vagueness of all other calculations.

The most interesting question is, How much may we estimat with the greatest degree of probability, the population of the natio not Russian?

I. *Poles.*

Poland in 1772, according to the researches of Count Tschatzk a learned Polish author, had a population of 14 millions. Polan was entirely divided 23 years after between Austria, Prussia, an Russia.

a. Gallicia fell to the share of Austria. This province is divide into eastern and western, with Bukowine. An enumeration mac in 1807 gives to western Gallicia,

Males	646,712
Females	660,550
Inhabitants	1,307,262

To eastern Gallicia, with Bukowine,

Males	1,863,904
Females	1,922,004
Inhabitants	3,785,908

The sum total is,

Males	2,510,616
Females	2,580,554
Inhabitants	5,091,170

b. Prussia had in the departments of Lithuania, Posen, Kaliscl Warsovia, Bialistok, and Plotz,

Inhabitants of towns	537,074
Inhabitants of the country	2,035,615
Inhabitants	2,572,689

c. Russia had, according to General Opperman, in 1796,

Inhabitants, at the first division of Poland, in 1773	1,226,966
at the second, of 1793	3,745,663
at the third, of 1795	1,407,402
Inhabitants	6,379,031

These provinces form at present seven governments: Vitebsk an Mohilew, or White Russia; Wilna and Grodno, or Lithuania Minsk; Volhynia; and Podolia. White Russia was acquired i

1778, the other governments in 1793, and they were increased at the last division of 1795.

The statements respecting the population of these governments which I have consulted are,

Vitebsk and Mohilew	736,376 males
Vilna and Grodno	796,633
Minsk	576,027
Volhynia	568,578
Podolia	576,027
	3,253,641

This statement does not include the females.

2. Two tables of the total number of inhabitants made in 1803 and 1804 by the Minister of the Interior:—

Vitebsk	Males	302,286	In 1803—Total 599,696
	Females	297,410	
	Males	343,716	In 1804—Total 674,340
	Females	330,624	Difference 74,644
Mohilew	Males	403,219	In 1803—Total 800,459
	Females	397,240	
	Males	403,614	In 1804—Total 800,995
	Females	397,381	Difference 536
Vilna	Males	470,064	In 1803—Total 925,207
	Females	455,143	
	Males	465,224	In 1804—Total 925,270
	Females	460,046	Difference 63
Grodno	Males	300,278	In 1803—Total 591,060
	Females	290,782	
	Same number repeated in the table for 1804		
Minsk	Males	438,455	In 1803—Total 868,393
	Females	429,938	
	Males	431,586	In 1804—Total 858,526
	Females	426,940	Difference 9,867
Volhynia	Males	563,700	In 1803—Total 1,083,536
	Females	519,836	
	Males	564,586	In 1804—Total 1,086,768
	Females	522,182	Difference 3,232
Podolia	Males	555,499	In 1803—Total 1,092,025
	Females	536,526	
	Males	579,215	In 1804—Total 1,136,085
	Females	556,370	Difference 44,060

Total for 1803—	Males	3,034,501
	Females	2,926,875
	Inhabitants	5,961,376

Total for 1804—Males	....	3,088,219
Females	....	2,984,825
Inhabitants	....	6,073,044

According to these data the population would have gained 111,668; but it is more probable that this apparent augmentation is the effect of repeated mistakes in the enumerations. The difference between the statement of General Opperman in 1796, and the population of 1804, is considerable, amounting to 305,987. In general the first enumerations give the smallest sums; but in this case we see the contrary. It is probable that during the first years there took place a silent emigration, similar to what happened in the Tauride—an event pretty common in countries newly occupied. But the difference appears too great to be accounted for in this way. In the ministerial statements of 1803 and 1804 it is observed, "The real number of inhabitants is greater than is marked in these statements; for it has been found that the numbers given by the governors do not much exceed those which give only the persons comprehended in the revisions. We may safely add 20,000 inhabitants to every government."

If we add, then, for the seven governments, 140,000, the total number in 1804 will be 6,213,044; which differs by 164,987 from the number of inhabitants assigned in 1796.

From these data Austria appears to have in her Polish provinces,

	5,091,170 inhabitants
Prussia	2,372,689
Russia	6,213,044
	13,876,903

If we consider the imperfection of such enumerations, we may suppose the round number of 14 millions, which Count Tschatzki gave in 1772. Hence it appears that the population of Poland is stationary.

Russia received by the peace of Tilsit and of Vienna about 600,000 new Polish subjects; so that the total number of Polish Russians amounts to 6,800,000.

II. *People of the Finnish race.*

a. Inhabitants of ancient Russian Finland. At the fourth revision of 1782 there were reckoned,

Males	93,234
Females	93,266
Inhabitants	186,500

Among whom were 64,543 peasants of the crown, and 2,207 belonging to individuals: total of peasants, 66,750.

At the fifth revision, of 1796, there were 92,684 males; among whom were 57,379 peasants of the crown, 2,028 belonging to domains, and 30,000 to individuals: sum total of peasants 89,447.

A partial enumeration of 1797 gave 89,188 peasants.

The first commission for the affairs of Finland, established on the 19th of May, 1803, indicated 64,074 peasants of the crown, and 28,000 belonging to individuals: total 92,074. This appears the most exact number.

The statements of the total population presented to the Minister of the Interior, differ very little from the preceding statement. They make

In 1803*—	Males	92,195
	Females	90,196
	Inhabitants	182,391
In 1804—	Males	94,397
	Females	87,993
	Inhabitants	182,390

The first of these numbers is evidently the revisionary peasants, and confirms the remark just made; for at the fifth revision of 1796 there were found 3,247 males in the other classes: namely, clergy, 327; nobles, 531; freemen not included in the other classes, 117; merchants, 408; artisans, 1854. We cannot quite double this number for the females, because all the tables show that the number of females is inferior to that of males; but we may at least add 60,000 to the population of 1803. The statement of 1804 is rather imperfect; but it approaches nearest the truth.

As there are few Russians in Finland, we may suppose 182,000 Finns in that government, according to the data of 1803.

An enumeration made in Sweden in 1805 gives to Finland formerly Swedish 895,773 inhabitants: namely,

Nobles	2,534
Clergy	4,019
Burghers	11,454
Peasants	713,285
Persons not included in these classes	164,480

Thus the sum total in old and new Finland is 1,077,773 inhabitants.

b. The *Ischores*, or Finns of Ingria, constitute the great majority of the inhabitants of the country in the government of Petersburgh.

At the fifth revision there were in this government,

Peasants belonging to individuals	122,913
———— domains	14,678
———— the crown	30,827
	168,418

A table drawn up for the tax on spirits in 1803 gives almost the same number, though otherwise distributed:

Peasants belonging to individuals	123,055
——— domains	1,421
——— the crown	43,558
	168,034

Another report respecting the distribution of salt gives 168,602 peasants.

The statements of the general population of this government give for 1803 the number of revisionaries,

Males	168,900
Females	170,989
Inhabitants	339,889

The statement for 1804 (excluding the capital) is more exact, namely,

Males	268,748
Females	270,920
Inhabitants	539,668

We may therefore reckon the Ischores inhabiting the government of St. Petersburgh at 330,000.

The Ischores inhabit the northern parts of the government of Novgorod. Their number is reckoned in the circle of Tichwin at 15,000 men, in the circle of Belosersk at 10,000, and in the circle of Kirilow at about the same. Hence there exist in this government about 35,000 males, or 70,000 individuals of this race.

The Ischores, or rather the Finns of Carelia, were the old inhabitants of the government of Olonetz. At present they constitute no more than a third of the population of the country, which according to a table drawn up in 1804 amounted to 91,482 males; so that 30,000 males, or 60,000 Ischores of both sexes, is their amount.

From these data the most probable amount of the Ischores is as follows:

Ischores of St. Petersburgh	330,000
——— Novgorod	70,000
——— Olonetz	60,000
	460,000

c. The *Laplanders* of Archangel amount to 1200 families, or about 4800 individuals. This number will not appear too great if we consider the imperfect state of the enumeration of the nomades.

d. The *Esthes*, a people of the Finnish race, are spread over Livonia. The Livonians, the ancient inhabitants of the country, exist in a very small number upon the little river Salis. They have been confounded among the Lettes, a Sclavonian people, and among the *Esthes*. Of these last there were in the circle of Walk about 2,000 males; in the circle of Werroe, 28,394 in the country, and 126 in the town; about 10,000 in the country in the circle of Dorpat, and 1,625 in the town; in the circle of Fellin, 18,388 in the country, and 76 in the town; and, finally, in the circle of Pernau, 33,158: making a total of 93,767 males, or 187,534 individuals. These data are not new; but the population in Livonia having made little progress, in 1792 there were

Males	268,891
Females	269,580
	538,471

And in 1800—	Males	285,498
	Females	285,421
		570,914

We may make use of them as terms of approximation.

As for the Esthes of Esthland there were in 1795,

Peasants belonging to individuals	93,156
——— domains	1,638
——— the crown	6,173
	100,967

In 1797 there were reckoned 99,484 peasants, almost the same number.

In 1803 the whole of the population was,

Males	107,357
Females	105,591
Individuals	312.948

By doubling the data for 1795 we would have the number of 389,468 for the Esthes in Livonia.

e. The *Syrjaenes*, a tribe of Finns in the government of Wologda and Perme, do not exceed a few thousands.

f. The *Permaeques*, the *Wogules*, and the *Wotjaeques*, according to the statements in the tables of the governments of Tobolsk and Tomsk, amount to 2,017 males, or about 5,028 individuals.

g. The *Tschouwasches*, the *Morduanes*, and the *Tscheremisses*, ding to the financial table of 1795, amount to the number of males: namely, 144,006 Tschouwasches, 62,732 Mor-
088 Tscheremisses, or in all about 511,652 individuals.
iaques on the Ob, in the government of Tobolsk,
males: the *Tepteri* and *Bobilei*, a Finnish and
government of Perm, to 1,838 males; making
or 41,058 individuals.

The result of these data respecting the Finnish nations is as follows:—

Finns	1,077,773
Ischores	460,000
Esthes	389,468
Tschuwasches, Morduanes, and Tscheremisses.	511,651
Permaeques, Wogules, and Wotjaeques	1,028
Syrjaenes	3,000
Laplanders	4,800
	2,492,779

We may estimate the whole Finnish people therefore at two millions and a half.

III. *Tartars.*

a. Tartars of Kasan.—The statements of the population of this government in 1802 make it 47,801 males; a number approaching to that of Georgi (t. iii. p. 363,) obtained from the third revision, of 1763: namely 48,712 males. We may estimate the total at 95,602.

b. Tartars of Astrachan.—From the statements of 1802 the nomades Tartars amounted to 6,703 families, or about 26,812 individuals; the Tartars dwelling in fixed habitations, 9,508 males; making a sum total of 45,828 individuals.

c. Tartars of the Crimea and Ecatherinoslaw.—According to Pallas (Voyage dans les Provinces Meridionales de la Russie, t. ii. p. [illegible]7,) they amount to 120,000 males. The statements respecting the Tartar population of this government are very imperfect. The Tartars have long been in the habit of withdrawing themselves from the revision. On that account the estimate of Pallas is the most probable.

d. The Tartars of Perme, according to Mr. Bakarewitsch, in his work, entitled, Statistical Description of Siberia drawn up from the Reports made to the Minister of the Interior, published in 1810, amount to 5,629 males, and the Tartars of Tobolsk to 25,820; making together 31,440 males.

e. Tartars of Caucasus.—The returns of 1802 mark only those of Tarkow, to the number of 1200 families.

From these data there are,

Tartars of Kasan	95,602
Astrachan	45,828
Crimea and Ecatherinoslaw	240,000
Siberia	62,898
Caucasus	4,800
Total	449,128

But as all the statements of the population of these people show that the number of females is inferior to that of males, it may be necessary to strike off about 30,000 on this account. Their number

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will then be conformable to the statement of Mr. Storch, who, estimating the Russian Tartars at 200,000, and those united to Russia by the treaties of peace of 1774, 1783, and 1791, at 214,318, makes the sum total amount to 414,318 individuals.

The Baschkines, the Metscheraeques, the Boucharzi, the Taschkinzi, the Jakoutes, and the Kirgises, are likewise of the Tartar race.

According to the statements of 1802 and 1803 there were

Baschkines and Metscheraeques, in the government of Perm	13,508 males
Boucharzi and Taschkinzi, in the governments of Tobolsk and Tomsk	2,895
Jakoutes of Tobolsk	258
Jakoutes of Irkoutch	50,676
	67,337

Or 134,674 individuals; so that the sum total of Tartars is 583,802. But we must strike off 30 or 40,000 on account of the deficiency of women. This will reduce the number to 550,000.

IV. *Inhabitants of Caucasus subject to Russia.*

The statements of 1803 make their number

Males	37,658
Females	32,203
Individuals	69,861

The statements of 1804 make

Males	34,849
Females	29,240
Individuals	64,089

V. *Samojedes.*

The statements of 1803 and 1804 give 3,000 families of Samojedes.

The American tribes are not numerous. The numbers given in the above-mentioned statements are,

Alioutari	246
Joukagires	505
Karagassi	163
Kamtschadales	1,782
Koraeques	1,224
Kouriles	100
	4,020 males

Or 8,040 individuals. These, with 12,000 Samojedes, make 20,040 individuals.

Nothing can be more imperfect than the enumerations of th tribes in the north of Siberia. Several are not even known. E in the present year (1810) several tribes of Jakoutes sent a depu tion to Tobolsk bearing the act of their submission; for, say th we have learnt that our brethren are happy under your domini His Majesty our august Emperor ordered each of these deputies receive a sabre as an honorary distinction.

VI. *Tribes of Mongoles and Mantschoux.*

According to the statements made to the Minister of the Inte there are,

Buraetes or Bratzki	58,767
Calmucks of Tobolsk	1,158
Calmucks of Astrachan, or 13,000 families	50,000
Mongoles	96
Tunguses of Irkoutzk	12,832
Tunguses of Tobolsk	1,999
Lamuti	976
Tschapogiri	308

And besides 23,090 individuals who were exempt from the impos about 140,225 males, or 298,450 individuals.

The known number of all these tribes does not surpass 300,0 individuals.

I add a general statement respecting all the nomades of Rus In 1803, according to the statements laid before the Minister the Interior, they amounted to

Males	652,000
Females	472,000
Individuals	1,124,000

All the reports show that these tribes have a deficiency women; but it is true likewise that the women are not so carefu registered as the men, because they pay no imposts.

The preceding results give us the following table of the peo subject to the Russian empire that are not Russians:

Poles	6,800,000
Finlanders	2,500,000
Tartars	550,000
Caucasians	60,000
Samojedes, and other Siberians	300,000
	10,210,000

This is the probable number resulting from the statements present in our possession. But it is proper to remark that all t statements respecting the population of Russia, being drawn up financial or military purposes, are very exact respecting those i cluded in the class of revisionaries, but very inaccurate as far as respects the other classes, and consequently upon the whole always below the truth. We may therefore reckon in the empire ten millions and a half of subjects who are not Russians.

The number of inhabitants at present in Russia is 41,253,483, and this number is certainly a minimum.

According to these data the number of Russian inhabitants amounts to 31,043,483, and the foreigners subject to the empire do not exceed one quarter of the whole people. This is a proportion very advantageous for the ruling nation. The variations in the total amount will not alter this proportion.

And these 31 millions of Russians have the inestimable advantage of a concentrated population, while the other nations are spread over a prodigious extent of country. The Russian nation forms the centre of this immense empire, it inhabits the best cultivated provinces, and is the best situated for communicating both by land and water. The south of Russia begins to be peopled by the surplus of the Russian population. All these advantages double the force of the Russian nation, and ensure it the most decided preponderance.

Article VIII.

Outline of Dr. Berzelius's Chemical Nomenclature.

By the Editor.

I promised, in a preceding number of the *Annals of Philosophy*, to present my readers with a sketch of the chemical nomenclature of Berzelius; because some knowledge of it seems to me requisite for understanding the important papers with which this excellent and indefatigable chemist has already enriched this Journal. It was contrived by the author in consequence of a new edition of the Swedish Pharmacopœia, and has been adopted in that work. An account of it was published by the author in the Journal de Physique for 1811, vol. lxxiii. p. 253: and in Gilbert's Annalen der Physik for 1812, vol. xlii. p. 37. From these two works I derived my knowledge of it.

The nomenclature is in Latin. It will be necessary merely to exhibit a list of the terms, with an English translation of each, and an explanation where it seems necessary.

I. IMPONDERABILIA. IMPONDERABLE BODIES.

Electricitas positiva	Positive electricity.
negativa	Negative electricity.
Lux	Light.
Caloricum	Caloric, or heat.
Magnetismus	Magnetism.

II. PONDERABILIA. PONDERABLE BODIES.

	I. SIMPLICIA.	SIMPLE BODIES.
1.	*Oxygenium*	*Oxygen.*
2.	*Metalloida*	*Metalloids.*
	Sulphuricum	Sulphur.
	Phosphoricum	Phosphorus.
	Muriaticum	Muriatic radicle.
	Fluoricum	Fluoric radicle.
	Boracicum	Boron.
	Carbonicum	Carbon.
3.	*Metalla*	*Metals.*
	Arsenicum	Arsenic.
	Molybdænum	Molybdenum.
	Chromium	Chromium.
	Wolframium	Tungsten.
	Tellurium	Tellurium.
	Osmium	Osmium.
	Tantalum*	Columbium.
	Silicium	Silicon.†
	Titanium	Titanium.
	Zirconium	Zirconium.
	Stibium	Antimony.
	Bismutum	Bismuth.
	Stannum	Tin.
	Iridium	Iridium.
	Platinum	Platinum.
	Aurum	Gold.
	Rhodium	Rhodium.
	Palladium	Palladium.
	Hydrargyrum	Mercury.
	Argentum	Silver.
	Plumbum	Lead.
	Niccolum	Nickel.
	Cuprum	Copper.
	Cobaltum	Cobalt.
	Uranium	Uranium.
	Zincum	Zinc.
	Ferrum	Iron.
	Manganium	Manganese.
	Cerium	Cerium.

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* In this name Berzelius has not done justice to the original discoverer. Dr. Wollaston has shown that columbium and tantalum are the same metal. The first was discovered by Mr. Hatchett some years before tantalum was announced by Mr. Ekeberg. Mr. Hatchett's name, therefore, as first imposed, and equally good, ought to be retained.

† I conceive that this substance ought to have been placed along with boron and carbon. We have no proof whatever that it is a metal.

Yttrium	Yttrium.
Beryllicum	Glucinum.
Aluminium	Aluminium.
Magnesium	Magnesium.
Calcium	Calcium.
Strontium	Strontium.
Barytium	Barytium.
Natrium	Sodium.
Kalium	Potassium.
Ammonium	Ammonium.

The preceding arrangement is the supposed electrical order of the various bodies, beginning with oxygen, which is decidedly negative. Those metals that form acids are placed first, and those that form only bases are placed last.

II. COMPOSITA. COMPOUNDS.

A. *Composita Inorganica.* *Inorganic Compounds.*

a. *Ammonium cum Oxygenio.* *Ammonium with Oxygen.*

Hydrogenium	Hydrogen.*
Ammoniacum	Ammonia.
Nitrogenium	Azote.

b. *Suboxida.* *Suboxides.*

By *suboxide* is meant a body containing so little oxygen as neither to constitute an acid nor a salifiable basis. I suspect several of his supposed suboxides do not exist.

Suboxidum kalicum	Suboxide of potassium.
natricum	———————sodium.
plumbicum	———————lead.
zincicum	———————zinc.
ferricum	———————iron.
arsenicum	———————arsenic.
carbonicum	Carbonic oxide gas.
phosphoricum	Oxide of phosphorus.

c. *Oxida.* *Oxides.*

Oxides are bodies that form salifiable bases, or combine with other oxides without possessing acid properties. When the same base forms two oxides, the first is distinguished by a termination in *osum*, the second in *icum*.

Oxidum kalicum l. kali	Potash.
natricum l. natron	Soda.
baryticum l. baryta	Barytes.

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* Berzelius has since changed his opinion respecting this substance, as may be seen in a paper of his inserted in the second volume of the *Annals of Philosophy*, p. 365.

Oxidum stronticum l. strontia	Strontian.
calcaricum l. calcarea.	Lime.
magnesicum l. magnesia	Magnesia.
aluminicum l. alumina	Alumina.
beryllicum l. beryllia	Glucina.
yttricum l. yttria	Yttria.
cerosum	Deutoxide of cerium.
cericum	Peroxide of cerium.
manganosum	Protoxide of manganese.
manganicum	Deutoxide of manganese.
ferrosum	Deutoxide of iron.
ferricum	Peroxide of iron.
zincicum	Oxide of zinc.
uranosum	Protoxide of uranium.
cobalticum	Protoxide of cobalt.
niccolicum	Protoxide of nickel.
plumbicum	Yellow oxide of lead.
cuprosum	Protoxide of copper.
cupricum	Peroxide of copper.
argenticum	Oxide of silver.
hydrargyrosum	Protoxide of mercury.
hydrargyricum	Red oxide of mercury.
palladicum	Peroxide of palladium.
rhodicum	Oxide of rhodium.
auricum	Peroxide of gold.
platinicum	Peroxide of platinum.
iridicum	Oxide of iridium.
stannosum	Protoxide of tin.
stannicum	Peroxide of tin.
stibiosum	Protoxide of antimony.
stibicum	Deutoxide of antimony.
bismuticum	Oxide of bismuth.
zirconicum	Zirconia.
silicicum	Silica.
tantalicum	Oxide of columbium.
osmicum.	———osmium.
telluricum	———tellurium.
chromosum	Protoxide of chromium.
molybdicum	———molybdenum.
sulphurosum	———sulphur.
sulphuricum*	Deutoxide of sulphur.
nitrosum	Nitrous oxide gas.
nitricum	———gas.
hydrogenicum	Water.
d. *Acida.*	*Acids.*
Acidum chromicum	Chromic acid.

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* Berzelius conceives these two oxides to be formed by the action of oxymuriatic acid on sulphur. We have no proof of their existence.

Acidum molybdosum	Molybdous acid.
molybdicum	Molybdic acid.
arsenicosum	Arsenious acid.
arsenicum	Arsenic acid.
carbonicum	Carbonic acid.
boracicum	Boracic acid.
fluoricum	Fluoric acid.
phosphorosum	Phosphorous acid.
phosphoricum	Phosphoric acid.
muriaticum	Muriatic acid.
oxymuriaticum	Hyperoxymuriatic acid.
nitrosum	Nitrous acid.
nitricum	Nitric acid.
sulphurosum	Sulphurous acid.
sulphuricum	Sulphuric acid.

e. *Superoxida.* *Superoxides.*

By superoxides are meant bases combined with so great a quantity of oxygen that they cease to be capable of uniting with acids.

Superoxidum kalicum	Peroxide of potassium.
natricum	——— sodium.
manganicum	——— manganese.
cobalticum	——— cobalt.
niccolicum	——— nickel.
plumbosum	Red oxide of lead.
plumbicum	Peroxide of lead.
hydrargyricum	——— mercury.
muriatosum	Oxymuriatic acid.
muriaticum	Euchlorine gas.

As the remainder of the nomenclature contains nothing that will not be intelligible without an explanation, I shall defer the insertion of it for the present.

Article IX.

Astronomical and Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude, 51° 32′ 40·3″ North. Longitude West in Time 6″$\frac{82}{100}$.

April 30, Emersion of Jupiter's 1st Satellite	10h	14′	54·2″	Mean Time at H.W.
	10	15	01	Ditto at Greenwich,

Magnetical Observations.

1814.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
April 18	8h	43′	—°	—′	—″	1h	40′	24°	23′	33″	—h	—′	—°	—′	—″
Ditto 19	8	43	24	12	40	1	47	24	22	46	6	25	24	16	21
Ditto 20	8	45	24	13	32	1	45	24	24	37	6	25	24	16	00
Ditto 21	8	45	24	13	52	1	45	24	25	41	6	45	24	17	10
Ditto 22	8	50	24	12	47	1	55	24	24	54	6	45	24	14	31
Ditto 23	8	45	24	12	45	1	45	24	25	15	7	05	24	16	33
Ditto 24	8	45	24	13	17	1	45	24	22	40	—	—	—	—	—
Ditto 25	8	45	24	12	03	1	55	24	22	45	6	35	24	18	07
Ditto 26	8	50	24	14	00	1	50	24	21	23	6	40	24	15	17
Ditto 27	8	40	24	11	53	1	45	24	22	41	6	15	24	15	50
Ditto 29	9	00	24	13	42	1	50	24	22	41	6	30	24	15	41
Ditto 30	8	40	24	15	27	1	50	24	22	26	6	40	24	16	15

1814. Mean of Observations in April.	Morning	at	8h	45′.....	Variation	24°	12′	53″	West.
	Noon	at	1	48	Ditto	24	23	53	
	Evening	at	6	29	Ditto	24	15	30	
Ditto in March.	Morning	at	8	52	Ditto	24	14	29	West.
	Noon	at	1	52	Ditto	24	23	08	
	Evening	at	6	11	Ditto	24	15	33	
Ditto in Feb.	Morning	at	8	47	Ditto	24	14	50	West.
	Noon	at	1	52	Ditto	24	20	58	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at	8	52	Ditto	24	15	05	West.
	Noon	at	1	53	Ditto	24	19	03	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
1813. Ditto in Dec.	Morning	at	8	53	Ditto	24	17	21	West.
	Noon	at	1	53	Ditto	24	19	49	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	42	Ditto	24	17	42	West.
	Noon	at	1	54	Ditto	24	20	24	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	Ditto	24	15	41	West.
	Noon	at	1	59	Ditto	24	22	53	
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	Ditto	24	15	46	West.
	Noon	at	2	02	Ditto	24	22	32	
	Evening	at	6	03	Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	Ditto	24	15	58	West.
	Noon	at	2	02	Ditto	24	23	32	
	Evening	at	7	05	Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	Ditto	24	14	32	West.
	Noon	at	1	50	Ditto	24	23	04	
	Evening	at	7	08	Ditto	24	13	56	
Ditto in June.	Morning	at	8	30	Ditto	24	12	35	West.
	Noon	at	1	33	Ditto	24	22	17	
	Evening	at	7	04	Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	Ditto	24	12	08	West.
	Noon	at	1	37	Ditto	24	20	54	
	Evening	at	6	14	Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	Ditto	24	09	18	West.
	Noon	at	0	59	Ditto	24	21	12	
	Evening	at	5	46	Ditto	24	15	25	

Magnetical Observations continued.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
May 1	8h	45′	24°	15′	07″	1h	45′	24°	22′	56″	7h	05′	24°	16′	13″
Ditto 2	8	30	24	12	53	1	45	24	21	47	6	35	24	17	07
Ditto 3	9	10	24	14	25	1	45	24	22	42	6	30	24	14	39
Ditto 4	8	35	24	12	58	1	45	24	24	08	6	35	24	16	52
Ditto 5	8	40	24	10	53	—	—	—	—	—	—	—	—	—	—
Ditto 6	8	45	24	11	47	1	50	24	22	44	6	40	24	16	29
Ditto 7	8	55	24	15	05	1	50	24	23	04	6	35	24	16	29
Ditto 8	8	40	24	15	43	1	40	24	21	28	6	55	24	15	20
Ditto 9	8	40	24	12	20	1	40	24	23	19	6	25	24	18	35
Ditto 10	6	45	24	14	00	1	50	24	23	04	6	15	24	17	25
Ditto 11	8	55	24	13	22	1	45	24	18	53	6	30	24	17	36
Ditto 12	8	50	24	11	38	1	55	24	22	02	6	30	24	16	48
Ditto 13	—	—	—	—	—	2	00	24	21	26	6	35	24	14	25
Ditto 14	8	50	24	17	35	1	50	24	20	31	6	35	24	16	00
Ditto 15	8	45	24	12	11	1	45	24	18	33	6	35	24	14	42
Ditto 16	8	40	24	12	12	1	40	24	21	14	6	30	24	16	57
Ditto 17	8	40	24	13	08	1	45	24	11	52	6	30	24	15	44

ARTICLE X.

ANALYSES OF BOOKS.

A Treatise on New Philosophical Instruments for various Purposes in the Arts and Sciences, with Experiments on Light and Colours. By David Brewster, LL. D. F.R.S. and A. Edin.

THIS treatise is divided into five books.

Book I. treats of new micrometers for measuring small angles and distances in the heavens, and of the fibres most proper for these instruments.

Book II. treats of instruments for measuring angles when the eye is not at their vertex.

Book III. treats of instruments for measuring distances.

Book IV. contains the description of instruments for viewing objects under water; for measuring the refractive and dispersive powers of bodies. It comprehends also a very extensive series of experiments on the refractive and dispersive powers of various solid and fluid substances, and an account of new properties communicated to light by transmission through diaphanous media, and by reflection from the polished surfaces of opake and transparent bodies.

Book V. treats of new telescopes and microscopes, and contains a series of experiments on the action of refracting media upon the differently coloured rays.

In order to convey to the reader some idea of the instruments and methods described in this volume, and of the experimental results contained in the fourth and fifth books, we shall direct his attention to those which appear to us of most importance, and endeavour to give as intelligible an account of them as can be done without the aid of figures.

The various micrometers which have hitherto been used may be divided into two kinds, viz. those in which one image of the object is formed, and those in which two images are formed. The single image, or the wire micrometer, consists of two parallel wires, which are made to approach to, or to recede from, each other, by the revolution of a fine screw, containing 50 or 100 threads in an inch. These wires are separated till they exactly comprehend the object to be measured, and the number of revolutions of the screw which are required to bring the wires together becomes a measure of the angle subtended by the object, the value of one or more revolutions having been previously determined by experiment. This instrument in its best state is not only very expensive, but is extremely complicated, easily deranged, and liable to numerous sources of error, which have been pointed out by Bradley, Herschel, and other astronomers.

The principle of the new micrometer invented by Dr. Brewster is to have one or more pieces of wires *absolutely fixed* in the field of the telescope, and to separate them by an *optical* instead of a *mechanical* contrivance; for it is obviously the same thing whether the wires are opened to embrace the sun's diameter, or the sun's diameter magnified till it fills the space between the wires. This change, however, upon the magnitude of the object must be effected in a part of the telescope anterior to the wires. In order to accomplish this, a second object glass is made to move between the principal object glass and its focus, by which means the magnifying power of the instrument, and consequently the angle subtended by the wires, may be constantly changed. When the object glasses are in contact, the angle subtended by the wires is a maximum; and when they are at their greatest distance the angle is a minimum, and every intermediate angle between these two is measured by a *scale of equal parts equal to the focal length of the principal object glass.* In this construction the imperfections of the screw, the error arising from the uncertainty of the zero, from the bad centering of the lenses, from the want of parallelism in the wires, and from the minuteness of the scale, are completely removed.

The principle of the preceding micrometer applies most happily to the Gregorian and the Cassegrainian reflecting telescopes; and, what at first sight may appear paradoxical, these instruments may be converted into a very accurate micrometer, almost without the aid of any additional apparatus. A moveable object glass is not necessary, as in the former case, for the magnifying power of these reflecting telescopes may be varied merely by varying the distance between the eye-piece and the great speculum.

The same optical principle constitutes the foundation of the *new* *divided object glass micrometer.* In the old micrometer of this construction, invented by Savery, two semi-lenses were made to separate from, and approach to, each other by a fine screw; and when the two images of the object were in contact, the distance of the centres of the semi-lenses was a measure of the angle which it subtended. In Dr. Brewster's micrometer, however, the semi-lenses, fixed at an invariable distance, are made to move between the object glass and its focus, so that the two images can easily be brought into contact, and the angle measured upon a scale of equal parts as large as the focal length of the object glass.

The *luminous range micrometer*, which is entirely a new instrument, is intended to measure the angle subtended by two luminous objects. By pushing in the eye-piece the two luminous points are swelled into circular images of light; and when these images touch one another, their angular distance is indicated npon a scale of equal parts.

For an account of the other micrometers, namely, the circular mother-of-pearl micrometer, the rotatory micrometer, and the eye-piece micrometer, we must refer the reader to the work itself.

In the last chapter of this book the author discusses the subject of micrometrical fibres, and proposes to astronomers to employ glass fibres, for the purpose of removing the error arising from the inflexion of light.

The instruments described in Book II. cannot easily be explained without figures. The most important of them is a reflecting geniometer for measuring the angles of crystals, and a new angular wire micrometer. The *reflecting geniometer* is an instrument of very extensive use, not merely in measuring the angles of crystals, but in all optical experiments where the angles of prisms require to be ascertained. The *angular wire micrometer* is intended for the same purposes as the position micrometer invented by Dr. Herschel, which consists of two wires, one fixed, and the other moveable, crossing one another in the centre of the field, and forming every angle from 0 to 180°. In this construction it is very easy to read off the angle formed by the two wires; but it is obvious that all the observations must be made on one side of the centre of the field; that the real scale of the instrument is measured by the semidiameter of the field; and that in the case where the vertex of the angle is without the field, it is incapable of measuring it.

In order to get rid of all these defects, Dr. B. places both the fixed and the moveable wire at a little distance from the centre of the field, so that their intersection is variable. By this contrivance a longer radius is obtained in the mensuration of the angle, and the observations may be made near the very centre of the field; but while these advantages are gained, there is apparently a great difficulty in judging the value of the angle upon the scale. By the aid of a beautiful property of the circle, in which the angle formed by two lines placed in a circle is equal to half the sum, or half the difference of the arches which they intercept according as the lines intersect one another within the circle or without it, the value of the angle is easily obtained; and by dividing the circular scale into 180° instead of 360°, and fixing the zero of the scale in a particular manner, the angle is read off with the very same facility as if the wires had crossed each other in the centre of the field.

The other instruments described in this chapter are a *double image goniometer*, a *diagonal telescope*, and *new protractors* for measuring angles.

The instruments described in Book III. are chiefly the applications of the micrometers in Book I. to military and naval telescopes for measuring angles and distances, with the methods of constructing the scales, and rules for using the instruments. These instruments are,

1. A micrometrical telescope for measuring distances.
2. A double image telescope and coming-up glass, for measuring distances at sea.
3. A luminous image telescope for measuring angles and distances during night.
4. Instruments for measuring inaccessible distances at one station.
5. Optical instruments for measuring inaccessible distances at one station.

The Fourth Book is principally devoted to the description of instruments, and methods employed in a very extensive series of experiments on light and colours.

In the second chapter the author has described a new method of measuring the refractive powers of solid and fluid bodies, and has given copious tables of the refractive powers which he has by this means ascertained. The method consists in introducing a portion of the substance between the object glass of a compound microscope and a plate of parallel glass. By this means a concave lens of the substance is formed: the focal length of the object glass is diminished; and in order to obtain distinct vision, it is necessary to place the object at a greater distance from the object glass. The distance therefore of the object from the object glass, when distinct vision is procured, becomes a measure of the refractive power. By this method the author obtained the most transparent lenses of aloes, pitch, opium, bird-lime, asafœtida, dragon's blood, caoutchouc, and many other substances through which light had never before been regularly refracted.

The tables of refractive powers that had hitherto been published never contained more than 60 or 70 substances, but those which are given in the present work contain about 300 measures of refractive powers. Many of these results are highly interesting to the chemist and the natural philosopher; and in the *Annals of Philosophy*, vol. ii. p. 302, we have already had occasion to notice some of the most important. This chapter also contains the account of a method of measuring the refractive power of solid fragments.

The third chapter is occupied with the description of a new instrument for measuring the *dispersive powers* of solid and fluid bodies, and with an account of the experiments which were made with it. Till within these few years the subject of dispersive powers, perhaps the most curious and useful in physical optics, has been investigated solely with the view of discovering achromatic combinations for the improvement of the telescope. The dispersive powers of different kinds of glass, and of a few fluids, were numerically ascertained; but no attempt was made to investigate the subject as an important branch of physics. Dr. Wollaston had the merit of beginning this interesting inquiry; and he determined the order of dispersive powers for 33 substances, without, however, giving any numerical estimate of their magnitude. By means of the instrument which has been mentioned, Dr. Brewster has ascertained in numbers the dispersive powers of 137 substances, the greater part of which were never before examined, and has obtained many results of a most unexpected and singular kind.

The fourth chapter of the work contains a series of experiments on the polarization of light by reflection and transmission, on the depolarization of light by transparent bodies, and the colours exhibited by mica and topaz when exposed to polarized light. This new branch of optics owes its existence to M. Malus, and has been diligently prosecuted since his death by the author of the present work. In the *Annals of Philosophy*, vol. iii. p. 6, we have already had occasion to give an account of the leading results of these experiments, and must therefore content ourselves with referring the reader to the numbers which we have now mentioned.

The fifth and last book is occupied with an account of new telescopes and microscopes.

In the first chapter the author gives an account of a series of experiments on the action of various bodies on the differently coloured rays, made with a view to the improvement of the achromatic telescope. Clairaut and Boscovich had ascertained, by unquestionable experiments, that in every achromatic telescope formed of crown and flint glass a considerable quantity of colour remained uncorrected, and that this uncorrected colour, or secondary spectrum, as it has been called, arose from the same coloured spaces having different magnitudes in equal spectra formed by the two kinds of glass. In consequence of this difference of action on the differently coloured rays, it is impossible to produce a telescope *perfectly achromatic* by flint and crown glass. Boscovich has shown how the uncorrected colour may be *diminished*, or how three of the colours have been united, by using three substances having different refractive and dispersive powers; but this method has never been carried into effect, and is of no practical use. Dr. Blair obtained the same results with Boscovich, and has pointed out a very ingenious method of removing the uncorrected colour by a double combination of fluid lenses; but this method is evidently too complicated, and will probably never be attempted by any practical optician.

In prosecuting these experiments Dr. Brewster has examined the action of a great variety of transparent bodies upon the differently coloured rays of light; but owing to the extreme delicacy of the results, he did not attempt to express them in numbers, though he has pointed out a method by which this may be effected. The general results of these experiments are given in the following table, which shows the order of the substances that form spectra, in which the red and green spaces are most contracted, and the blue and violet ones most expanded: that is, the substances are arranged inversely according to their action upon green light:—

Oil of cassia,
Sulphur,
Balsam of Tolu,
Carbonate of lead,
Oil of aniseeds,
Oil of sassafras,
Opal coloured glass,
Oil of cummin,
Oil of cloves,
Oil of lavender,
Canada balsam,
Oil of turpentine,
Flint glass,
Calcareous spar,
Oil of almonds,
Crown glass,
Gum arabic,
Alcohol,
Ether,
Leucite,
Blue topaz,
Fluor spar,
Nitrous acid,
Muriatic acid,
Rock crystal,
Water,
Sulphuric acid.

From these results it follows that the action of transparent bodies on the green rays *in general* diminishes as their dispersive power increases, and that the sulphuric acid exceeds all transparent substances, that have yet been examined, in its action upon the green rays, while oil of cassia exerts the least action upon them of any known substance.

In applying these conclusions to the improvement of the achromatic telescope, the author lays down the following maxims:— 1. The practical optician should always select flint glass with the least dispersive power. 2. The difference between the dispersive powers of the crown and flint glass should be as small as possible. By attending to these points the uncorrected colour will be the least possible, and the performance of the instrument greatly improved.

In the course of these experiments Dr. B. discovered what he calls a *tertiary spectrum*, produced by varying the inclination of the first prism to the incident rays. This new spectrum appears even when the two prisms are formed out of the same substance.

The instruments described in the remaining chapters of this book are,

1. A new compound microscope for examining objects of natural history, and capable of being rendered achromatic.
2. A new polar microscope, which can be rendered achromatic.
3. New fluid microscopes.
4. Adjusting microscopes, for seeing at two different distances at the same time.
5. Opera glasses and night glasses upon a new construction.

ARTICLE XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

ON Thursday, the 28th of April, the remainder of Dr. Brewster's paper on the optical properties of mother-of-pearl was read. This substance possesses the property of polarizing light in a way different from all other bodies. In crystallized bodies the opposite polarization of the two images is always related to some axis or fixed line in the primitive form; but no such relation exists in mother-of-pearl. In uncrystallized bodies the reflected pencil is always polarized in an opposite manner to the refracted pencil; but in mother-of-pearl a single plate possesses the property of polarizing the whole of the transmitted light at an angle of incidence of 60°; and the transmitted pencil is polarized in the same manner as the reflected pencil.

At the same meeting a paper by Captain Henry Kater was read, describing a new method of dividing circles, and other astronomical instruments. The method of Bird was used in dividing astronomical instruments till it was superseded by the new method of Troughton, of the perfection of which the mural circle at the Greenwich Observatory exhibits an admirable proof. Captain Kater conceives that the method of Troughton admits of simplification. He considers his own method as possessed of the greatest possible simplicity, and as very accurate. It would be impossible to render this method intelligible without figures. We shall not therefore attempt to give an outline of it.

On Thursday, the 5th of May, the remainder of Captain Kater's paper on the method of dividing astronomical instruments was read.

On Thursday, the 12th of May, a paper by Dr. Benjamin Heyne on the Indian method of oxydizing silver by means of the juice of jatropha cureas, and on the milk of plants, was read. A piece of silver is heated to redness, wrapped in the leaves of any kind of tree, and then quenched in the juice of the jatropha moluccana. This process is repeated about twenty times, taking care never to fuse the silver. The metal becomes quite brittle, and crumbles to powder between the fingers. Dr. Heyne tried the same process, substituting water instead of the vegetable juice, and a similar effect was produced. From Dr. Heyne's account of the above process I suspect that the silver is not oxidized; but merely rendered brittle, and reduced to a fine powder; probably by combining with something which exists in the vegetable juice employed, or rather in the cow-dung in which the silver is heated. The only known oxide of silver is a dark greenish brown powder, which is reduced to the metallic state by a very moderate heat. If the vegetable juice merely communicated oxygen, it is obvious that that principle would be driven off every time the metal was heated to redness; so that the process would never advance: but if the vegetable juice or cow-dung employed supplied sulphur, or any analogous principle, we can see how the repeated heatings would facilitate the combination, and how fusion would retard it.

Dr. Heyne's observations on the milk of plants were curious and valuable, but of too miscellaneous a nature to admit of abridgment here.

On Thursday, the 19th of May, a paper by Dr. Brewster was read on the optical properties of hot glass. The author discovered that glass, when heated nearly to redness, polarizes and depolarises light, and forms two images, one of which coincides with the other. Hence it is analogous to doubly refracting crystals. The beautiful coloured rings produced by topaz were not perceptible in this case; but it occurred to Dr. Brewster that glass, in order to produce them, must be in a state of fusion. As it was not possible to examine its optical properties in that state, Dr. Brewster had recourse to the glass tears formed by dropping melted glass into cold water, on the supposition that from the sudden cooling of the outer coat the interior part of these tears would be in the same state as melted glass, or at least their ultimate particles at the same distance from one another as in melted glass. He found accordingly that these tears produced the coloured rings in question. He found that this tear has regular axes of crystallization, the axis of the conical tail, and a line perpendicular to it, corresponding with the short and the long diagonals of a rhomboid of calcareous spar.

At the same meeting a paper by Captain Kater was also read, containing farther experiments on the light of the Gregorian and Cassegrainian telescopes. He examined the quantity of light passing through a glass lense before and behind the focus. He found the light behind the focus to that before as 1000 to about 650, or somewhat less in the greater number of experiments. These results approach to the degree of light observed in the Cassegrainian and Gregorian telescopes, and therefore serve to confirm Captain Kater's preceding observations.

At the same meeting a paper by Mr. Herschell was announced, on some remarkable properties of mathematical analysis. As this paper was not read, it is impossible to give any account of it.

LINNÆAN SOCIETY.

On Tuesday, the 3d of May, the remainder of Dr. Leach's paper, exhibiting a tabular view of four classes of animals, considered by Linnæus as constituting but one class, was read.

On Tuesday, the 24th of May, the following office bearers were chosen for the ensuing year:—

President—James Edward Smith, M.D. F.R.S.
Secretaries—Alexander Macleay, Esq. F.R.S.
Mr. Richard Taylor.

OLD COUNCIL.

James Edward Smith, M.D. F.R.S.
Samuel Lord Bishop of Carlisle,
Sir Thomas Gery Cullum, Bart.
Philip Derbishire, Esq.
Mr. James Dickson,
Aylmer Bourke Lambert, Esq.
Alexander Macleay, Esq.
Thomas Marsham, Esq.
William George Maton, M.D.
John Sims, M.D.
Edward Lord Stanley.

NEW MEMBERS OF COUNCIL.

William Elford Leach, M.D.
Daniel Moore, Esq.
Joseph Sabine, Esq.
Thomas Smith, Esq.
William Smith, Esq. M.P.

WERNERIAN SOCIETY.

On Saturday, the 5th of March, there was read an interesting paper on the middle granite district of Galloway, by Dr. Grierson. It appears, from the Doctor's observations, that this granite extends from eight to nine miles in one direction, and from three to four in another. It is coarse granular, sometimes porphyritic, but does not appear to be stratified. It is situate in the midst of distinctly stratified rocks, which on the east side of the granite mass dip easterly, on the west side westerly, or in both cases away from the granite; out on the north and south ends of the mass, the ends of the strata run directly towards it. The rock which rests immediately on the granite is a particular variety of compact fine granular gneiss, and cotemporaneous veins of the granite are to be observed shooting from the granite into this variety of gneiss. The gneiss seems to be connected with greywacke and greywacke slate, which are by far the most frequent of the stratified rocks in this tract of country. Limestone, hitherto a desideratum in the transition rocks of Galloway, was discovered by Dr. Grierson, in greywacke, near to Dalmellington; and it is highly probable that *workable* beds of limestone will be found among the stratified transition rocks of Galloway. The Doctor also described several beds of felspar-porphyry, which he noticed in the greywacke of this part of Scotland.*

At the next meeting, Professor Jameson gave an account of overlying primitive formations, as the first part of a dissertation on overlying rocks in general. From a series of observations which were made in the Highlands of Scotland, it appears that many of the primitive overlying sienite, granite, and porphyry formations of mineralogists, are not so in reality, but are thick conformable beds of these rocks which rise more or less above the surrounding strata. At the same meeting Professor Jameson described the Criffle district of granite and sienite, situate in the county of Galloway. These rocks occupy a considerable tract of country, and rise to the height of 1895 feet above the level of the sea; they are not distinctly stratified, and exhibit many interesting appearances, of apparent fragments, of cotemporaneous veins, and transitions into porphyry. The rocks which rest immediately on the granite or sienite are fine granular compact gneiss, slaty sienite, horneblende rock, and compact felspar rock. These rocks alternate with each other, and sometimes even with the sienite or granite; and cotemporaneous veins of granite are to be observed shooting from the granite into the adjacent stratified rocks. At the *Needle's Eye* on the west of Galloway, the Professor observed very fine examples of cotemporaneous veins and masses of granite, &c. in compact slaty felspar; and the felspar itself points out a hitherto unsuspected connection of this mineral with certain kinds of clay-slate. On these rocks rest greywacke and greywacke slate, and sometimes transition porphyry; and it would appear, from Mr. Jameson's observations, that there is an almost uninterrupted transition from the gneissy rock into greywacke; and that when the felspar of the greywacke increases very much in quantity, becomes compact, dark coloured, and slaty, the greywacke at length passes into clay-slate.

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* The reader will find Dr. Grierson's paper printed at full length in the present Number of the *Annals of Philosophy*.—T.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute of France during the Year 1813.

(Continued from p. 392.)

New Memoirs on the Polarization of Light. By M. Biot.

The memoirs read this year by M. Biot, contain the developements and the consequences of those of which we have given an account in our preceding analyses. We cannot render our account of them intelligible without noticing some of the facts contained in the preceding ones. M. Biot had first considered the phenomena of coloration, first observed by M. Arago in thin plates of certain crystallized and uncrystallized bodies. These phenomena are not represented by the formulas given by Malus for polarization in Iceland crystal. M. Biot finds a general expression which embraces them all in every possible position of the plates. He shows the relation which subsists between these colours polarized extraordinarily by these plates, and those of the thin plates of uncrystallized bodies which Newton had observed, and which furnished him with the means of foretelling the colour from the thickness.

In his second memoir he explains the formulas which his experiments had suggested. He shows the kind of polarization which they indicate. These do not take place merely in thin plates, for the author has shown the method of developing long series of colours in plates several centimetres thick, by the increase of their axes. He gives a general expression for these phenomena. He shows that in the crystal that produce them, the luminous molecules perform, round their centres of gravity, oscillations, of which he determines the extent, the duration, the velocity, as well as the law of the forces that produce them; and coming from this principle to the phenomena, he finds by this inverse method of proceeding the very same formulas which he had at first drawn directly from observations.

He establishes by experiment the way in which the oscillations must continue in passing from one plate to another. Then he deduces from the theory all the phenomena of colours which plates laid above one another can produce, supposing their axes to cross at any angle whatever, when they are exposed perpendicularly to a polarized ray; and he shows that all these corollaries agree perfectly with observation.

He deduces equally from his theory the phenomena which take place in case of oblique incidence both by refraction and reflection. For the plates parallel to the axis of crystallization the agreement with experiment is complete. He extends his results to the cases when the axis is inclined to the surfaces of the plates at any angle whatever.

He observes the peculiar phenomena which plates of rock crystal present when cut perpendicularly to the axis of crystallization, and exposed to a ray polarized under a perpendicular incidence. He shows that the forces which produce these phenomena are independent of those which produce the oscillations. He proves, by numerous and exact experiments, that the luminous molecules acquire in traversing these plates peculiar properties which they retain afterwards when they pass into space, and which do not consist merely in a new disposition of their axes, but in a true physical modification. The forces which then act upon them do not make them oscillate, but turn in a continued motion round their centre of gravity. He explains the laws of this rotation; he shows how it may be succesively directed from right to left and from left to right. Finally, by inclining the plates, he shows how it is opposed and destroyed by the force which produces the oscillations when this force can develope itself.

He shows that in plates of mica, when they are regularly crystallized, the polarizing forces emanate from two lines or axes, one of which is situated in the plane of the plates and the other is perpendicular to them; hence it follows, that the theory of oscillations does not immediately apply to the phenomena of mica, as under the perpendicular incidence of the influence of this last axis it is null. Considering the simultaneous action of these two forces when the plate is inclined, he deduces from them all the complex phenomena which the mica presents under different inclinations.

To confirm this theory by a striking proof, he assembles plates of sulphate of lime and of rock crystal, so as to imitate the systems of forces which he has discovered in mica. The resulting phenomena are absolutely the same with those exhibited by the mica.

In another memoir, M. Biot deduces from his theory the constancy of the colours which plates of sulphate of lime exhibit under all incidences when they are inclined upon a polarized ray, so that their axis of crystallization makes an angle of 45° with the plane of incidence. He shows also, by calculation, on what the change of colour depends, which takes place when it is removed from that position, and in what order it ought to take place according to the different movements given to the plate.

Hitherto all the experiments of M. Biot, as well as the theory deduced from them, apply only to the cases in which the double refraction is very weak. However, in a memoir read this summer to the Society of Arcueil, he proved that the extraordinary refracting force of Iceland crystal acts upon the different luminous molecules in the same order as that of other crystals. And he has been able to ascertain from theory how it is necessary to proceed, in order to attenuate the repulsive force of Iceland crystal, so as to make it produce the phenomena of colours similar to those of other crystallized bodies that he had observed. He has succeeded in the same way with arragonite, the double refraction of which is also very strong. Further, by crossing these two substances with plates of sulphate of lime, he has produced colours under all incidences, and in circumstances when they were very far from giving them naturally. This shows, that the luminous molecules commence oscillating in them, as well as in other substances, before assuming a fixed polarization. These results, which complete the theory at the same time that they confirm it, constitute the object of the last memoir read by M. Biot.

(To be continued.)

Article XII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Method of graduating Glass Tubes.*

I promised last summer to give a description of my method of graduating glass tubes; but have not hitherto been able to find room for it in the *Annals of Philosophy*. My method is as follows :—

1. Provide a quantity of mercury more than sufficient to fill the tube to be graduated. Take its specific gravity with great care; let us suppose it to be 13·399.

2. Suppose our object is to graduate the tube into 100ths of a cubic inch. At the temperature of 60° the 100th part of a cubic inch of pure water weighs 2·525 grains troy. The 100th part of a cubic inch of our mercury weighs $2 \cdot 525 \times 13 \cdot 399 = 33 \cdot 83$ grains.

3. Weigh out 33·83 grains of mercury with all possible accuracy. Put it into a small glass capsule. Prepare a glass tube, about a foot in length, almost capillary in the bore, open at both ends, and terminating at one extremity in a point. Apply your mouth to the upper extremity of this tube, and draw into it the whole of the mercury which you have weighed out, taking care that it fills the lower part of the tube exactly without leaving any vacuity. Before you remove your tongue from the upper orifice of the tube, apply your finger to the lower orifice, and then with the edge of a small triangular file mark on the tube the upper surface of the mercury. The bore of the tube should be so fine that the mercury ought to occupy about three inches in length. This measure being thus made, the principal part of the labour is over.

4. Take a slip of writing paper about ¼th inch in breadth, and as long as the tube. Apply gum water to one side of this slip, and paste it on the tube, wrapping round it a piece of twine from one end of the tube to the other, so as to keep it firmly fixed in its place. Set it aside till the paper be quite dry, then remove the twine; for the paper will now adhere firmly to the tube.

5. Place the tube in a perpendicular direction, upon a table, with its open end uppermost. Put the mercury whose specific gravity you have determined, into a cup. Draw into your tube, previously graduated as a measure, a quantity of mercury, till it stands exactly at the mark on the tube. Pour this quantity into the tube, and let an assistant draw a pencil mark upon the paper, corresponding with the upper surface of the mercury. Pour in a second measure of mercury, and mark its surface on the paper with a pencil line as before. Proceed in this way till you have filled your tube; and a little practice will enable you to proceed with great dispatch, without any diminution of precision.

6. You have now your tube graduated by pencil lines drawn on the slip of paper pasted to it. Take a fine triangular file, moisten one of its edges so as to enable you to cut through the paper without using any violence. Cut through the first pencil mark, and by drawing the file along the tube you easily make a fine and distinct line on the tube exactly under the pencil mark. Cut in this way through all the pencil marks, and make every fifth and tenth division twice as long as the others. In this way you may cut through all the pencil marks in a few minutes, and when the paper is re-

moved you will find your tube very neatly and distinctly graduated.

7. Mark with a diamond the figures denoting every tenth degree, and the whole is finished.

II. *On the Term Fluid Ounce.*

I have received the following communication from a gentleman who subscribes himself W. L. C. I may notice that the term *fluid ounce* to which he objects, was introduced by the London College of Physicians in the last edition of their Pharmacopœia, in order to remedy a defect which had previously existed in this country. Weights and measures were formerly denoted by the same names. Fluid ounce, if I remember right (for I have not the Pharmacopœia at hand) means the twelfth part of a pint, and is therefore equivalent to 2·406 cubic inches. I am quite of my correspondent's opinion, that such expressions should be avoided in a chemical dissertation, and that cubic inches, which is a measure free from ambiguity, ought always to be employed in preference. At the same time it is not surprising that Mr. Brande, who is Lecturer to the Apothecaries, and who must in consequence be very familiar with their measures, should make use of such expressions.

"The term '*fluid ounce,*' frequently employed by a learned and ingenious chemist, in his *Researches on the Blood,* and other papers contained in the latter volumes of the Philosophical Transactions, appears liable to some objections in point of philological accuracy: it is used to signify a measure of any liquid equal in volume to an ounce of water, whereas the obvious sense of the words is, an ounce in weight of a substance in a fluid state. It would be preferable to express the quantity in cubic inches, and instead of one fluid ounce, to say $1\frac{728}{1000}$ cubic inches; instead of two fluid ounces, $3\frac{456}{1000}$ cubic inches. In order to avoid fractions, measuring vessels marked with the cubic inch and its multiples, ought to be employed in place of those marked with the ounce of water." W. L. C.

III. *Antilunar Tide.*

(To Dr. Thomson.)

SIR,

It appears to me that the two principles of gravity and projectile force account in a very satisfactory manner for the antilunar tide.

If a fluid body move in a circle round any centre, (its centrifugal force counteracted by gravity) it will assume a form nearly spheroidal.* For its centre of gravity is that part where the two forces are counterbalanced; consequently, the further part of it has greater centrifugal than centripetal force, and would fly off, were it not restrained by the general mass; and the nearer part of it has more centripetal than centrifugal force, and would fall towards the centre, were it not restrained by the same cause. For the same reason that a fluid body turning on its axis assumes the form of an oblate spheroid, does a fluid body, turning round a centre, out of itself, assume the form of an oblong spheroid, gravity and projectile force being the sufficient and obvious cause of each. And the rotation of such body, upon any other than its longer axis, must, as it presents a new part towards the centre round which it revolves, constantly occasion two opposite tides.

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* The greater the distance between two revolving bodies, other things remaining the same, the more nearly will their forms approach to spheroids.

It is the necessary consequence of these laws, that there should exist on our earth one lunar and one antilunar, one solar and one antisolar tide, I am, Sir, with much respect,

Your most obedient Servant,

Newcastle, April 29, 1814. W.

IV. *Prize Essay of the Royal Medical Society of Edinburgh.*

The Royal Medical Society propose as the subject of their Prize Essay for the year 1815, the following question:—

"The comparative specific caloric of venous and arterial blood."

A set of books, or a medal of five guineas value, shall be given annually to the author of the best dissertation on an experimental subject proposed by the Society; for which all the members, honorary, extraordinary, and ordinary, shall alone be invited as candidates.

The dissertations are to be written in English, French, or Latin, and to be delivered to the Secretary on or before the first of December of the succeeding year to that in which the subjects are proposed; and the adjudication of the prize shall take place in the last week of February following.

To each dissertation shall be prefixed a motto, and this motto is to be written on the outside of a sealed packet, containing the name and address of the author. No dissertation will be received with the author's name affixed; and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

V. *On the Method of preserving Ships.*

The following valuable hints on this subject, so interesting at present, are too important to require any introductory observations from me.

(To Dr. Thomson.)

MY DEAR SIR, *Hackney Wick*, 17*th May*, 1814.

As peace has taken place, I beg leave to submit for consideration, a method of preserving such of our men of war as are building, and also those which are finished and may now remain on the slips. To accomplish this desirable purpose, a permanent roof should be built over each vessel, which would serve for after vessels, with gutters to carry off the rain; most of the treenails should be driven out, leaving no more than are sufficient to confine the planks to the timbers: the auger holes being left open will cause a continual current of air, not only through the body of the vessel, but even through the heart of the timber. I have good reason to believe this would not only prevent the dry rot, but season the timbers, and thus preserve the external and internal planks and increase the durability of the ship: the driving out of the treenails would be attended with little expense, as they might be kept in store until wanted, and this mode of ventilation would in my opinion totally supersede the use of stoves, which are not only dangerous but expensive, and in some instances do more harm than good.

Perhaps, this proposition deserves the more attention, as the treenails have, in some instances, been made of American wood, which has produced the dry rot in the oak timbers through which they were driven, the dry rot having begun in the treenails.

Being on the subject of preserving the navy, I beg leave to propose as worthy of serious consideration, whether vessels, which are kept in still water, do not much sooner decay than those which float in running water; should this idea be well founded, the naval departments would ascertain the fact before any large sum of money be expended in extensive docks, which might produce the great inconvenience of the ships being found unserviceable on an emergency.

I remain, my dear Sir, yours faithfully,
MARK BEAUFOY.

VI. *Query respecting Nails.*

(To Dr. Thomson.)

SIR,

The public will be benefited if any of your scientific readers, or correspondents, can give information through your monthly publication, how a nail can be made that will bear driving into oak, and drawing out if necessary, of metal, cast or malleable; if copper forms a part it must be alloyed not to injure iron; if iron, cast or malleable, they must be rendered less subject to corrosion when exposed to the sea water. I am,

Your constant reader and obedient humble Servant,
Liverpool, May 14th, 1814. P. S.

VII. *Lectures.*

The Summer Courses of Lectures on the Theory and Practice of Physic, Materia Medica, Chemistry, and the Clinical Lectures, by Dr. Roget and Dr. Harrison, will commence at the Medical Theatre, Great Windmill-street, on Monday, the 6th of June, at nine in the morning.

ARTICLE XIII.

New Patents.

MARC ISAMBARD BRUNELL, Chelsea, civil engineer; for a method of giving additional durability to certain descriptions of leather. March 12, 1814.

Matthew Murray, Leeds; for methods and improvements in the construction of hydraulic presses, for pressing cloth and paper, and for other purposes. March 12, 1814.

Emanuel Beatsen, Birmingham, gun-finisher; for an improvement to the locks and breeches of fire-arms, by rendering the pans of locks, and communication between the priming and loading of fire-arms, water proof. March 23, 1814.

William Alfred Noble, Riley Street, Chelsea, engineer; for an improved steam and fire engine, and new means of connecting or joining steam or water pipes together. March 23, 1814.

John Sparks Moline, London; for an improved method of tanning leather. March 28, 1814.

John U. Rastrick, Bridgenorth, Salop; for a steam engine on a new and improved construction. April 1, 1814.

Joseph C. Dyer, Boston, New England; at present in London; for improvements in machinery for manufacturing nails of various kinds. Communicated by a foreigner residing abroad. April 1, 1814.

James Wood, New Compton Street, London; for an improvement in the German flute; applicable also to the clarionet and bassoon. April 1, 1814.

George Smart, Westminster Bridge; for certain improvements in machinery for grinding corn and various other articles. April 1, 1814.

John Roberts, Drury Lane; for map-rollers and carriage-blinds, and other similar objects. April 7, 1814.

Isaac Mason, Wellonhall, Stafford; for a method of making stamped fronts for register stoves, ship stoves and other stoves, fenders, tea trays and other trays, mouldings and other articles in brass and other metals. April 7, 1814.

William Whitfield, Birmingham; for certain improvements in carriages. April 7, 1814.

John Read, Horsemonden, Kent; for means of raising and conveying water, steam, gas, or any other fluid by pipes of purified earth. April 18, 1814.

ARTICLE XIV.

Scientific Book in hand.

Mr. Singer proposes to publish annually a supplement to his "Elements of Electricity and Electro-Chemistry," under the title of "Annals of Electrical Science." It will consist of original experiments, and papers, by the author and his friends; and a full account of the progress of electrical discovery for the past year. The first number will most probably appear on the 1st of January, 1815.

ARTICLE XV.

METEOROLOGICAL TABLE.

1814.	Wind.	Barometer.			Thermometer.			Evap	Rain.
		Max.	Min.	Med.	Max.	Min.	Med.		
4th Mo.									
April 12	N	29·83	29·76	29·795	71	39	55·0		
13	S E	29·76	29·62	29·690	74	42	58·0		—
14	S	29·62	29·48	29·550	70	43	56·5		
15	S E	29·44	29·42	29·430	69	46	57·5		—
16	S	29.42	29·23	29·325	65	47	56·0		·30
17	S	29·54	29·23	29·385	62	40	51·0		—
18	S W	29·59	29·43	29·510	66	39	52·5		
19	S E	29·65	29·59	29·620	64	43	53·5		
20	S E	29·65	29·64	29·645	68	48	58·0	·78	·15
21	S W	29·73	29·64	29·685	62	39	50·5		·13
22	N W	29·96	29·73	29·845	57	35	46·0		
23	S W	29·96	29·74	29·850	59	42	50·5		·25
24	N W	29·80	29·74	29·770	55	41	48·0		3
25	N W	29·86	29·80	29·830	51	40	45·5		—
26	N W	30·10	29·86	29·980	55	33	44·0	·37	·13
27	N W	30·12	30·09	30·105	56	41	48·5		3
28	S W	30·09	30·04	30·065	52	43	47·5		·12
29	S E	30·00	29·98	29·990	60	46	53·0		—
30	S W	30·10	30·00	30·050	64	42	53·0		
5th Mo.									
May 1	Var.	30·12	30·10	30·110	68	48	58·0		
2	E	30·05	30·00	30·025	60	44	52·0		
3	E	30·00	29·83	29·915	63	41	47·0		
4	S E	29·83	29·70	29·765	57	36	46·5	·70	
5	Var.	29·70	29·28	29·490	53	40	46·5		—
6	Var.	29·70	29·28	29·490	63	45	54·0		·66
7	N E	29·95	29·70	29·825	67	45	56·0		—
8	W	30·14	29·95	30·045	69	43	56·0		
9	N E	30·27	30·14	30·205	57	35	46·0		
10	N E	30·42	30·27	29·345	55	33	44·0	·30	
		30·42	29·23	29·770	74	33	51·39	2·15	1·80

The observations in each line of the table apply to a period of twenty-hours, beginning at 9 A. M. on the day indicated in the first column. A denotes, that the result is included in the next following observation.

REMARKS.

Fourth Month.—12. *Cirrus* clouds, passing to the intermediate modifications. The dust is become so dry and light that the air is filled with it: this is in part to be ascribed to electrical action. 13. Various clouds appeared a. m. including the *Nimbus;* and a few drops of rain fell. Thunder clouds, p. m.: a coloured twilight. 14. Clouds of various modifications. 15. A few drops, a. m.: a swallow appeared on the wing: p. m. steady rain. 16. Much wind: showers. 17. Steady rain in the morning: wind S. hollow, and murmuring: after this large *Cumulus* clouds (beneath *Cirrus*), which in the evening rapidly evaporated or dispersed; an Aurora Borealis ensued, of which an account has been already given. (See the Remarks in last Number.) 18. Wet morning and evening: much *Cumulus* appeared to-day, intermixed with *Cirrostratus* in the region of its base, an appearance very unusual. 19. *Cirrostratus,* a. m.: day fine: a *lurid* sun-set, the disk showing enlarged, through spots and lines of *Cirrostratus.* 20. a. m. Clouds, beneath an elevated haze, in which were discernible streaks from N. to S.: rain in the night. 21. The clouds inosculating, a. m.: rain ensued in the evening. 22. Windy: *Cumulostratus*: a brisk evaporation. 23. Shower, p. m.: rain in the night. 24. Windy; small rain. 25. Overcast: showery. 26. After a shower, various clouds through the day, and a rainbow: at sun-set a beautiful *Cumulostratus* in the S. E. reflecting the splendour of the twilight. 27. Cloudy: rain to the S. W. 28, 29. Wet mornings. 30. A grey elevated sky.

Fifth Month.—1. Windy. 2. Overcast, a. m.: then fair, with *Cirrus.* 4. Cloudy morning. 5. The same: a windy and very wet day: the rain mixed at intervals with sleet and hail. 6. Showers, attended with the union of clouds in different strata. At sun-set an appearance of extensive rain in the E. with groups of *Cumulus* and *Cirrostratus* before it. In the N. and W. these had, intermixed and adhering, a transparent brown-red haze, distinguishable from the substance of the cloud, and which gave a pink tinge to the twilight, elsewhere of the usual lemon colour. At the same time a *Stratus* arose in the meadows. 7. A misty morning: while the earth presents the aspect of spring: the sky of late is quite autumnal. Showers again passed by in the E. 8. Cloudy morning: rain in the S. and E. *Cumulus* clouds, uniting with *Cirrus* above. 9. Overcast sky.

RESULTS.

Winds variable.

Barometer: Greatest height.................... 30·42 inches;
Least.................................. 29·23 inches;
Mean of the period .... 29·770 inches.
Thermometer: Greatest height .... 74°
Least.. 33°
Mean.. 51·39°
Evaporation 2·15 inches. Rain 1·80 inches.

TOTTENHAM, *Fifth Month,* 19, 1814. L. HOWARD.

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END OF VOL. III.

ERRATA.

Page 63, line 16, *for* $\frac{n^3 - 3n^2 + 2n}{6}$ *read* $\frac{n^3 + 3n^2 + 2n}{6}$: and *for* $\frac{\overline{n^2 - n} \times \overline{2n + 1}}{3}$ *read* $\frac{\overline{n^2 + n} \times \overline{2n + 1}}{3}$.

Page 80, line 8, *for* insulated *read* inosculated.
———, 5 lines from bottom, *for* epasm, *read* spasm.
———, 3 lines from bottom, *for* a production, *read* it productive.
Page 130, line 17, *for* centrifugal, *read* centripetal.
Page 215, 2 lines from bottom, *for* 50th, *read* 60th.
Page 334, line 2 from bottom, *for* electric *read* elective.

ERRATUM IN VOL. II.

(*Not before noticed.*)

Page 458, I have inadvertently ascribed the number 135° for the latent heat of water to Mr. Cavendish. It was Lavoisier that gave that number. Mr. Cavendish found the latent heat of water 150°. This alters the case entirely, and renders it next to certain that 140°, the number of Dr. Black, is nearer the truth than 135°, the number pitched upon by Mr. Leslie.

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